Agglomeration Spillovers and Persistence: New Evidence from Large Plant Openings

by

Carlianne Patrick
Georgia State University

Mark Partridge
The Ohio State University

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Abstract

We use confidential Census microdata to compare outcomes for plants in counties that “win” a new plant to plants in similar counties that did not to receive the new plant, providing empirical evidence on the economic theories used to justify local industrial policies. We find little evidence that the average highly incentivized large plant generates significant productivity spillovers. Our semiparametric estimates of the overall local agglomeration function indicate that residual TFP is linear for the range of “agglomeration” densities most frequently observed, suggesting local economic shocks do not push local economies to a new higher equilibrium. Examining changes twenty years after the new plant entrant, we find some evidence of persistent, positive increases in winning county-manufacturing shares that are not driven by establishment births.

Keyword*:* local economic development, agglomeration externalities, persistence

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Partrick: Associate Professor, Department of Economics, Georgia State University, cpatrick@gsu.edu
Partridge: Professor, Swank Chair in Urban Rural Policy, Department of AED Economics, The Ohio State University, partridge.27@osu.edu

1. Introduction

Luring new business through tax credits, grants, low-interest loans, and other financial incentives – collectively labeled economic development incentives – is foremost amongst the policies used by state and local governments to support local economies and jobs (Combes et al. 2010; Bartik 2012; Story 2012; Patrick 2016). The use of economic development incentives is growing. Current estimates of state and local incentives range from $45 billion to $90 billion (in 2015$) annually (Bartik 2017). Increasingly, these resources are directed towards extremely high-cost incentives packages for very large projects that promise significant job creation and investment (Schwartz 2018). In addition to the direct benefits of the new business, such generous incentives policies are justified based upon indirect (multiplier) effects, or spillovers, generated by the large, new entrant.

These large new establishments are expected to increase productivity and output at existing establishments, spur additional new business entrants, and, generally speaking, put the location on a higher, long-term growth path. Policymaker discussions surrounding recent competitions for Intel, Amazon HQ2, and Foxconn demonstrate these expectations. Yet, the lifespan of the average U.S. establishment is 10 years, regardless of age or size – a timespan that is often less than or equal the duration of the promised benefits and incentive package (Santa Fe Institute 2015). Long-term benefits of such firm attraction then critically rely on indirect spillovers.

The notion of long-lasting effects from a one-time shock reflects the economic concept of persistence. Persistence in the spatial distribution of economic activity or persistence in the effects of shocks is a prominent outcome in a large class of models incorporating localized increasing returns to scale (agglomeration externalities) or other forms of endogenous amenities (Lee and Lin 2018). This class of models also feature multiple equilibria. Following Krugman (1991), there are multiple equilibria in a locations' share of overall manufacturing in which the observed spatial distribution of activity depends upon the history of locational shocks. In such models, shocks of sufficient size can "push" a location past some specific-activity threshold to a new-equilibrium steady-state share of economic activity.

These models' theoretical potential for policy-induced change gives credence to the policymaker rationale for generously incentivizing large new plants, and there is also empirical evidence suggesting these shocks can be important. For example, studies that document long-run effects from temporary factors or shocks provide evidence supporting the existence of path dependence and multiple equilibria. Bleakley and Lin (2012) document the long-lasting effects of portage (water-way transhipment points) on the current spatial distribution of U.S. population despite more than a century of obsolescence. Similarly, Hanlon (2014) demonstrates that the large, temporary, negative shock to cotton supplies caused by the U.S. Civil War had long-run effects on the distribution of U.K cotton manufacturing. More recently, Allen and Donaldson (2018) demonstrate that spillovers from historical (as well as contemporaneous) shocks are empirically important in the U.S. context. Of course, this depends upon the strength of agglomeration spillovers and curvature of the (net) agglomeration function (Bleakley and Lin 2015; Davis and Weinstein 2002, 2008; Allen and Donaldson 2018).

The types of long-lasting change that arise from large shocks and strong agglomeration externalities are what underlies the rationale for attracting large plants. Yet, there is little empirical evidence available to support or undermine the notion of transformational change from successful attraction of a large plant. Using confidential Census microdata, we provide estimates that directly address the core reasoning inherent in local industrial policy. We ask three related questions: First, how consistent is the evidence that successful attraction of large plants generates significant agglomeration externalities in the form of productivity spillovers for nearby incumbent plants? Second, how does incumbent-plant productivity vary at different densities of "economically-close" plants, i.e. what is the shape of the agglomeration function? And, finally, do we find evidence of persistent changes in winning locations' share of manufacturing (manufacturing-industry) activity?

Greenstone, Hornbeck, and Moretti (2010) (GHM) provide the only existing microdata evidence on the agglomeration externalities associated with successful attraction of a highly-incentivized, large new plant. They estimate very large productivity spillovers associated with their set of 47 large plant openings and this suggests that their set of large plant shocks are ideal candidates to induce long-lasting effects. We start with their plant openings and then expand the analysis to other sets of incentivized plant openings as well as a set of randomly drawn large new plants.

We estimate significant cumulative increase in incumbent plant productivity after 5 years associated with the GHM MDP openings, albeit without the large mean shift estimated by GHM. However, these spillovers are econometrically identified by a unique subset of plants that continuously operate in counties that are both a winner and a loser for more than one county in the sample. After numerous robustness checks, they also appear to be unique to the particular MDP openings in the GHM sample and sensitive to empirical specification. Our results suggest much weaker spillovers associated with other highly-incentivized MDP openings, although consistent with the larger agglomeration literature. These findings support the idea that agglomeration externalities are a function of many offsetting negative and positive interactions, so that the addition of one establishment (even a very large establishment) is unlikely to cause large changes in the overall level of interactions.

Having established the short-run magnitude of large, new-plant spillovers, we nonparametrically estimate the effect of local plant density on plant output in a partially-linear regression model and instrument for density using deep lags of county market potential, while conditioning for other key location factors. We obtain a kernel-density estimate whose shape may be interpreted as the shape of the agglomeration functions with respect to four definitions of local plant density. In this way, we contribute the first microdata derived non-parametric estimates of the agglomeration function across different sources of agglomeration externalities.

Our semiparametric estimates of the agglomeration function indicate that the residual TFP is *linear* with interactions between economically-close plants for a range of densities most frequently observed in the data. This suggests minimal potential for long-lasting effects and multiple-equilibria characteristic of models featuring localized increasing returns to scale.

We look for empirical evidence of such long-run effects by comparing county-manufacturing output and county-manufacturing industry shares of national manufacturing from before the large plant opening with those 20 years after. Our findings indicate the MDP shocks have persistent, positive effects on winning-county output shares, but the data do not strongly support multiple equilibria. We also find no evidence of longer-term increases in establishment births in winning counties. Thus, our results suggest that even in the presence of significant spillovers for some incumbent plants and persistent effects on output, these large plant openings are an insufficiently large positive shock to push locations into a new equilibrium. This may be due to countervailing congestion forces or weaker than anticipated spillovers or both.

Our research contributes to a recent body of literature examining economic development incentives and, more specifically, highly-subsidized large firms (e.g., Gabe and Kraybill 2002; Slattery 2020; Kim 2018; Slattery and Zidar 2020; Freedman 2017). Slattery (2020) documents the potential for aggregate welfare gains from subsidized firms that is a function of the relationship between locations' incentive bids and welfare. She acknowledges the inherit issues in quantifying the locational benefits and this paper provides additional insights on those benefits. We do so by looking directly at short-run changes in TFP and long-run changes in output shares, firm births, and firm deaths.

We provide the only plant-level estimates of changes in short-run TFP other than GHM, with estimates directly comparable across the GHM and other sets of large plant openings. We also highlight the sensitivity of such estimates. In doing so, we answer the long-standing question about the generalizability of their results to local industrial policies targeting large establishments.

We also contribute the only microdata examination of the long-run effects of local industrial policies. Freedman's (2017) county-level investigation of the Depression-era Mississippi "Balance Agriculture with Industry" (BAWI) Program that started the current trends in local economic development policy is the only other study to examine longer-term effects associated with subsidizing large plants.

Through economic development policy and local spillovers associated with broader regional economic growth, our work relates to the long-running literature on local employment, output, and value-added (GDP) multipliers. Focusing on local employment multipliers (LEM), they equal the total local employment change after an exogenous increase of one local worker—e.g., a LEM of 2.5 implies that one newly-created exogenous *direct* job leads to another 1.5 new jobs elsewhere in the local economy.[3] Very large LEMs, for example, can support "big-push" development leaps or even multiple-equilibria outcomes.

The modern LEM literature dates to Daly (1940) for LEMs derived from economic-base models and from Isard (1951) for LEMs derived from interregional input-output models. The economic-base model assumes that local regions have a base (or traded-export) employment that is supported by local or nonbase employment. The corresponding LEM equals one base job plus the number of nonbase jobs supporting the base increase. Yet, the economic-base model suffers from key-theoretical shortcomings outlined in Tiebout (1956) and Kikenny and Partridge (2009). Mainly, its mercantilist approach assumes local growth only originates from local exports, along with the base model's failing to consider that a local trade surplus (deficit) means, by definition, that there is an equal-sized capital-account deficit (surplus) that also affects local activity. LEMs derived from interregional input-output models also face many theoretical concerns such as the absence of prices and the disregard of other offsetting "crowd-ins" or "crowd-outs."

Bartik (1991) began a renascence of statistically-estimated LEMs with his use of the employment-growth share from shift-share analysis as an exogenous instrument for overall local employment growth. The share variable also can be directly used as a labor-demand-shifter

[3]In simple settings, total new jobs can be decomposed into: (1) direct jobs, (2) indirect-jobs from input-output supply-chain, as well as tertiary-industry effects that support the direct- and supporting-industry effects, and (3) induced-jobs from the increased spending of workers on such items as groceries, entertainment, etc. Farren and Partridge (2015) using the IMPLAN™ input-output social-accounting-matrix software found that the LEM for coal mining in the three-core Virginia coal-producing counties equaled 1.74, i.e., 1=direct + 0.36 indirect + 0.38 induced. In reality, there are other positive/negative impacts from exogenous shocks due to productivity spillovers; higher factor prices crowding-out other firms; finite elasticity of local labor supply and net-migration; start-ups or firm deaths induced from the exogenous shock; dynamic agglomeration or congestion effects, etc.

variable in empirical models, from which the coefficient reflects the multiplier.[4] A key advantage of econometrically-estimated LEMs is *all* the positive and offsetting negative effects of an exogenous direct-employment shock are estimated, and hence, not constrained by stringent assumptions in other models. Morreti (2010) further accelerated the estimation of LEMs across a plethora of cases such as for traded goods, "high-tech," nontraded goods, oil and natural-gas jobs, business start-ups and small businesses, etc. Extensive reviews by Bartik and Sotherland,[5] Van Dijk (2018), and Osman and Kemeny (2021) conclude that econometrically-estimated county-level LEM multipliers realistically equal about 1.5 and about 2.0 for U.S. states. Detang-Dessendre et al. (2016) found similar multipliers for French local regions. Yet, LEMs in the 1.5 to 2.0 range suggest that local growth induced by exogenous demand shocks are unlikely to produce a transformative big-push shifting local economies to new-equilibria paths.

Besides economic development implications, we also contribute to the theoretical and quantitative-modelling literatures featuring agglomeration externalities or endogenous amenities by providing micro-founded nonparametric estimates of the agglomeration function and estimating the persistence parameter for a local shock (e.g., see Davis and Weinstein 2002, 2008; Bosker, Brakman, Garretsen, and Schramm 2007; Redding, Sturm, and Wolf 2011; Bleakley and Lin 2015; Lee and Lin 2018; Allen and Donaldson 2018).

Our findings also relate to the literature on the persistence of local-employment demand shocks (Bartik, 1993; Amoir and Manning 2018; Deltas et al. 2019). Our findings of persistent output effects without significant changes in births and death are similar to Bartik (1991, 1993)

[4]Shift-share analysis of local economies dates to U.S. military planning during World War II and remains a key practitioner tool (Dunn 1960). The local-share component equals the hypothetical employment-growth rate of the local economy if all of the local industries grow at their corresponding national growth rate—i.e., does the local economy have a composition of fast- or slow-growth industries. Bartik (1991) rationalizes the exogenous nature of the shift-share-component demand shock and explains that the key identification assumption to use it as an instrumental variable is that there are no *contemporaneous* local labor-supply changes that are associated with *lagged* local-industry composition (or at least after controlling for labor-supply variables to condition those effects from the residual). When used as a direct explanatory variable, the potential econometric concerns are reduced because it is not being used as an instrument, but not necessarily eliminated. For an overview of the econometric issues when using "Bartik" or "shift-share" variables, see Borusyak et al. (2022).

[5] For a summary of Bartik and Sotherland's findings and links to this research, see https://www.upjohn.org/research-highlights/what-are-realistic-job-multipliers, downloaded January 28, 2022.

and Amoir and Manning's (2018) results that employment rates modestly change in response to persistent local labor demand shocks. In other words, it appears that labor supply changes in net-migration and commuting patterns completely meet the persistent change in labor demand, leading to little long-run change in real wages. Together, our results and assuming perfect mobility of capital—which leads to the local return to capital equaling the national return—suggest limited welfare gains from highly-subsidized large-plant locations.

2. Incentivized Large Plant Openings

For our purposes, a firm's decision to open a new establishment or significantly expand an existing establishment is referred to as a case. The county in which the new establishment locates is referred to as the "winner" for each case. The counterfactual counties for each case are referred to as the "losers." A sample of cases includes the MDP as well as the "winner" and "loser" counties for each case. Identification relies critically on the selection of "losers" and is described in Section 3.1.

We examine four sets of MDP cases from multiple sources to determine the sets of potentially-incentivized plant openings. The analysis starts with replicating the primary spillover result in GHM. GHM base data relies on the "Million Dollar Plant" (MDP) sample outlined in Greenstone and Moretti (2003) (GM). According to the authors, they obtain the sample from 1982-1993 *Site Selection* magazine regular features "Million Dollar Plant".[6] *Site Selection* magazine is an internationally circulated business publication covering corporate real estate and economic development, which relies on state and local economic development organizations for advertising dollars.[7] The MDP series describes how high-profile plant location decisions were made, reporting the county where the plant located (the "winner"), and (sometimes) reports other

[6] The precise source of the sample is nuanced. See Appendix 1 of Patrick (2016) for more details.
[7] The magazine's primary audience is economic development and site-selection professionals engaged in firm recruitment and location assistance.

counties who were speculated to have been finalists in the site-selection process (the "losers"). Generally, to be listed by the magazine, competitions for MDPs had to be public knowledge, which creates an unknown selection bias.

GHM use 47 of 63 potential GM manufacturing cases in their analysis.[8] We obtained the restricted-access statistical programs from the Census Bureau to determine the subset of GM MDPs used in GHM and construct the 47 case subset using these programs. We refer to the GHM subset of GM MDPs as case set 1.

Patrick (2016) details the process of reproducing GHM's sample from the primary-source documents and notes additional potential cases that appear in the magazine during the study period that were *not* included in GHM's sample (sometimes with both a winner and loser). We therefore expand the set of potential large-plant shocks to include these additional cases, so that all firm location decisions appearing in the magazine from 1982-1993 are considered, regardless of whether the article details the "loser."

We supplement the *Site Selection* magazine cases a database of incentivized plant locations provided the non-profit Good Jobs First. Good Jobs First began collecting this information in 1988 and our *Site Selection* magazine cases start in 1982. We collect data from the Good Jobs First database for 1988-1997, as well as a set for 1988-1993. We retain only new plant locations (or expansions) that have a reported subsidy value in the data and further restrict this sample to highly incentivized, large establishments. We define "highly-incentivized" as having received a minimum of $250,000 in public inducements. We define "large" as promising at least 50 jobs or a minimum of $1,000,000 in new capital investment.

We create two additional case sets by combining all *Site Selection* magazine and Good Jobs First cases for the periods 1982-1993 and 1982-1997. The combined sets provide better geographic coverage as well as larger sample sizes than when considering either source alone. The latter is particularly important for nonparametric and persistence exercises. We refer to these

[8] The GM MDP sample includes 82 total cases, but only 63 are manufacturing cases that can potentially be used the analysis.

as case-sets 2 and 3, respectively.

Finally, we create a set of random large-establishment openings directly from Census microdata. We randomly sample large new firms appearing over the sample period, where large is defined as having initial employment above the 95^{th} employment percentile for all new establishment births over 1982-1997. This set provides a nice benchmark to assess whether "new" firms in the Site Selection or Good Jobs First samples are different than a random "new" large firm regarding productivity spillovers.

Table 1 summarizes the case sets described above.

[Insert Table 1 approximately here]

Table 2 presents the MDP-shock characteristics used in the analysis by case set. These represent a subset of the potential cases in each case set. We selected the subset of case set 1 cases retained for analysis using the restricted-access GHM replication code provided by the Census Bureau. The subset of other case sets used in the final analysis was determined using the following criteria: 1) there is an establishment in the SSEL or LBD owned by the reported firm in the two years prior or three years after the plant opening announcement; 2) the SSEL/LBD establishment is located in the reported winning county or city; 3) there were incumbent plants in the winning county.

It is clear from Table 2 that the highly-incentivized openings and expansions reported by *Site Selection* magazine and Good Jobs First are very large compared both to the winning counties in which they are located and compared to an average large new entrant in the micro data. Column 1 reports that the GHM MDPs have (deflated) output that is, on average, 1.23 times larger than the initial value of output for all manufacturing establishments in the winning county, and (deflated) value-added that is 1.25 times larger. These plants employ 2,645 workers on average with average (deflated) payrolls of $143,000,000. The MDP ratio of other worker payroll to production worker payroll, which provides a potential measure of the human capital shock, is approximately 3 for the GHM MDPs. Adding additional MDPs that appear in the magazine as well as the highly-incentivized large plants openings reported in the Good Jobs First data yields the MDP case set 2, which produce output that is, on average, an even larger share of initial winning county output than the GHM MDPs with fewer employees and smaller payrolls. Adding the 1994-1997 MDP cases to the case set 2 MDP shocks produces case set 3, which represent, on average, smaller output shocks than case sets 1 and 2. However, case set 3 employment lies between the case set 1 and 2 employment with average payroll that is higher than both. The payroll standard deviation is quite large, suggesting that higher than average payrolls are associated with some very high-paying MDPs. The standard deviation of other worker to production worker payrolls is also quite large. Taken together, these indicate at least a few high human capital MDPs in case set 3.

[Insert Table 2 approximately here]

It is interesting to note that the randomly drawn large new births represent a much smaller share of winning county output than the average highly incentivized large plant. These randomly selected new entrants also have much smaller employment levels but the highest average payroll among the case sets – indicating that the random large plant has much higher average wages than the typical highly incentivized large plant. However, the extraordinary standard deviation for payroll suggests this may be driven by some very high-paying plants. The mean and standard deviation of case set 4's ratio of other worker payroll to production worker payrolls don't indicate extraordinary non-production human capital shocks; thus, it is possible that these are particularly high-paying (high-skilled) production jobs.

3. Estimates of MDP spillovers on incumbent plant productivity

3.1. Counterfactual selection

For comparability, we replicate the key spillover result in GHM. The GHM identification strategy relies on firms' revealed rankings over potential locations as reported in *Site Selection* magazine's regular feature "Million Dollar Plant" (MDP). While the authors make a strong *prima facie* case for the quasi-experimental research design, Patrick (2016) finds evidence that the revealed "losers" are sub-optimal counterfactuals, with approximately one-third being affected by treatment (the firm closed a plant in the losing county in order to open the new plant). As shown in Appendix Table A4, exclusion of losers where the MDP's firm closes a plant substantially reduces the estimated effects. We therefore explore results using alternative identification strategies described more below. The alternative strategies determine "losers" by geographic proximity to the "winner" and matching on observables and unobservables captured by industry locational advantage. In this study, "losers" must be located within a specified distance (100-250 miles) of the winning county (calculated as the distance between centroids) for each case.[9,10] As a robustness check, we repeat this process for "losers" drawn from 50-100 miles away from the treated counties. The geographic proximity restriction conforms to the typical manufacturing site-selection process that first selects a specified geographic region, and then second, picks the specific locale within the larger region. Close geographic proximity ensures shared factor markets. It also minimizes the possibility that differences in input prices are captured in the productivity estimates (Atalay 2012).

3.1.1. Revealed rankings

We recreate the GHM counterfactual counties by using the replication code provided by the Census to identify their sample of MDPs, applying the criterion described above, and then match those to the losers listed in the appendix of GM. This process yields the same number of cases and losing counties reported in GHM.

3.1.2. Geographically-proximate propensity score matches

Our primary alternative strategy identifies "losers" by matching on observables. Each case's

[9] Patrick (2016) uses 50-100 miles. As a robustness check, she analyzes all outcomes using matches located within 100-250 miles of the winning county. The results were qualitatively and quantitatively similar.

[10] Henderson (2003) finds no evidence of significant agglomeration spillovers between firms beyond county borders. Using 100-250 miles excludes adjacent counties and any possibility of confounding MDP spillovers; yet counties are still close enough to reflect large unobserved productivity shocks such as transportation upgrades and human capital influxes that are not attributable to the MDP.

counterfactuals are the nearest two propensity-score neighbors to the winning county within 100-250 miles. The covariates in the propensity-score matching-model are drawn from important determinants in agglomeration and site-selection research. The agglomeration literature suggests economic size, density, industry composition, transportation, wages, historical population growth, past employment growth, and other urbanization economies influence spillover effects (see Rosenthal and Strange 2004 for a review). Site-selection studies suggest many of these same factors influence the actual selection of the new facility's location (Brouwer *et al*. 2002; Guimaraes *et al*. 2003; Devereux *et al*. 2007). Thus, the observable matching estimator is conditioned on the following known determinants of treatment and outcomes: total county population; presence of an interstate in the county; distance to the nearest metropolitan area; share of population that is working aged; minority share of total population; earnings per-employed worker; historical population and employment growth; the share of total employment in manufacturing, farming, services, FIRE, and military; and case fixed effects. The historical variables account for historical geographic and agglomeration effects that would be key omitted effects otherwise (Duranton and Turner, 2011). We match on covariate values three-years prior to the new-plant's opening. We use the county identifier associated with each plant in the ASM and CM to select counterfactual plants.

3.1.3. Geographically-proximate location-quotient neighbors

As another alternative identification strategy, we match on counties' MDP-industry locational advantage as measured by the location quotient. As shown by Guimarães et al. (2009), the location quotient may be interpreted within the Ellison and Glaeser (1997) dartboard framework for measuring locational advantage. As such, plants in counterfactual counties with similar MDP-industry location quotients as "winner" counties should have access to similar observable and unobservable locational advantages. The location quotient also has the advantage of being easily calculated for counties from publicly available data.

Specifically, we construct industry *i's* location quotient in county *c* in year *t* as:

$$LQ_{ict} = \frac{\frac{Emp_{ict}}{Emp_{ct}}}{\frac{Emp_{i,US,t}}{Emp_{US,t}}},$$

where industry is defined as the 2-digit SIC code. We then choose counterfactual counties for each case as the two counties within 100-250 miles of the winning county with the nearest location-quotient values to the winning-county's case-MDP industry, measured three years prior to the MDP opening. We then use the county identifier associated with each plant in the ASM and CM to select the counterfactual plants from these counterfactual counties.

3.2. Incumbent-plant samples

As discussed below, our spillover estimating equations require plant-level data on the value of output, building & equipment capital stocks, and material inputs. These data are available in the restricted-use Census Annual Survey of Manufacturers (ASM) and Census of Manufacturers (CM). The value of capital stocks is not collected every year in the ASM/CMF; however, capital expenditure data is collected for surveyed plants. Using the perpetual-inventory method, stock and investment data may be used to construct the annual capital-stock variables. Alternatively, Foster, Grims, and Haltiwanger (2013) (FGH) construct the ASM-CMF Total Factor Productivity dataset and Beta Version 1.0 is available to researchers. This data includes TFP, capital stock, real input and output, and deflator data for most firms in the ASM-CMF 1972-2010. We use the perpetual-inventory method and the raw ASM/CMF data to construct capital stocks for the GHM replication results. The remaining production-function estimates employ the FGH TFP Beta Version 1.0 capital stocks.

To avoid changes in sample composition driving the results, incumbents must continuously appear in the data during the pre- and post-opening periods. The ASM sampling scheme rotates smaller plants and samples larger plants with more certainty. This means that the sample of continuously-appearing incumbent plants is skewed towards larger and more economically-important plants than the average U.S. plant. Table 3 presents summary statistics for the sample of incumbent plants by "treatment" (winner) status. Comparing (log) output and (log) labor by winner status within case sets, the samples are very well balanced. Also interesting is that the incumbent plant samples are relatively similar, on average, across case sets despite being associated with different winner and loser counties. This likely reflects the plant types that appear continuously in the data due to the ASM sampling scheme. The average number of continuously-appearing incumbent plants per county does vary across samples and winner status.

[Insert Table 3 approximately here]

Table 3 also presents the (rounded) number of counties from which the winner and loser incumbent plant samples are drawn as well as the number of unique counties in the data. For example, Table 3 reports that for the GHM replication sample (Case set 1 Winners and GHM Losers), the treated incumbent plants are drawn from approximately 50 counties and the counterfactual incumbent plants from approximately 80 counties, with approximately 100 unique counties between the two groups of winning and losing counties. As discussed below, identification of the parameters of interest in the replication equations rests on having overlap between cases and counties (i.e., a winner and loser appearing in the same county for different cases or the same county appearing with the same winner/loser status for more than one case) and that there are plants that continuously appear in those same (winner/loser) counties for more than one case. Table 3 demonstrates that all incumbent-plant samples meet this requirement and therefore, identification of our parameters is possible.

3.3. GHM estimating equations

We begin with GHM's empirical specification. For incumbent plants in "winner" and "loser" counties for case *j*, we estimate log output *Y* in plant *p*, in industry *i* at time *t* as a function of inputs and the MDP effect with the following estimating equations:

(1) $\ln(Y_{pijt}) = \beta_1 \ln(L_{pijt}) + \beta_2 \ln(K^B_{pijt}) + \beta_3 \ln(K^E_{pijt}) + \beta_4 \ln(M_{pijt}) + \delta 1(Winner)_{pj} + \kappa 1(\tau \geq 0)_{jt} + \theta_1 [1(Winner)_{pj} \times 1(\tau \geq 0)_{jt}] + \alpha_p + \mu_{it} + \lambda_j + \varepsilon_{pijt}$

(2) $\ln(Y_{pijt}) = \beta_1 \ln(L_{pijt}) + \beta_2 \ln(K^B_{pijt}) + \beta_3 \ln(K^E_{pijt}) + \beta_4 \ln(M_{pijt}) + \delta 1(Winner)_{pj} + \psi Trend_{jt} + \Omega[Trend_{jt} \times 1(Winner)_{pj}] + \kappa 1(\tau \geq 0)_{jt} + \gamma[Trend_{jt} \times 1(\tau \geq 0)_{jt}] + \theta_1 [1(Winner)_{pj} \times 1(\tau \geq 0)_{jt}] + \theta_2 [Trend_{jt} \times 1(Winner)_{pj} \times 1(\tau \geq 0)_{jt}] + \alpha_p + \mu_{it} + \lambda_j + \varepsilon_{pijt}$

where $\alpha_p, \mu_{it}, and\ \lambda_j$ are plant, industry, time, and case fixed effects, respectively, L_{pijt} is labor production hours, K^B_{pijt} is the value of land and building capital, K^E_{pijt} is value of equipment, and $M_{pijt}$is the value of materials. $Trend_{jt}$ is a time trend, $1(Winner)_{pj}$ is an indicator for being located in a winning county, $1(\tau \geq 0)_{jt}$is an indicator for *t* being a year after the MDP opened, and τ is year normalized such that $\tau = 0$ in the plant announcement year for each case.

The parameters of interest are θ_1 and θ_2. Under Model 1, θ_1 measures the difference in mean outcome for winning counties after successfully attracting an MDP. Thus, it is basically the difference-in-differences estimator of the "treatment" (winning) effect on incumbent-plant output. Model 2 allows for both a mean shift in outcome, θ_1, and a differential trend in outcome, measured by θ_2, in the winning county after an MDP opening.

It is important to note that Equations (1) and (2) include plant and case fixed effects as well as the standard treated-sample indicator, post-period indicator, and interaction variables in differences-in-differences equations. With plant and case fixed effects, the case fixed effect parameter is separately identified from the plant fixed effect parameters *only* when a set of plants corresponds to more than one case in the county. The δ parameter for $1(Winner)_{pj}$ is identified by within-plant variation in winner status. In other words, δ is identified by plants that are in a winning county for at least one case and the same county is also a losing county for at least one case. If no county appears as both a winner and loser in a sample of cases, then δ cannot be identified from Equations (1) and (2). Similarly, the plant and case fixed effects can only be separately identified for plants that appear as either a winner or loser for more than one case. The $1(\tau \geq 0)_{jt}$parameter κ is the mean difference in pre- and post-period within-plant TFP. The parameter κ can therefore only be identified for plants that appear both before and after the plant opening – or incumbent plants that remain for at least one period after the MDP opens. New entrants would not contribute to κ's identification.

GHM estimated Equations (1) and (2) for the sample of continuously appearing plants and we follow their methodology for comparability. With this strategy, the inclusion of counties that appear multiple times with either the same or different winner status is required for the county, case, and the winner-status dummy variables not to be perfectly collinear. This does not necessarily mean that the equations cannot be estimated, but it does change the interpretation of the results in light of the variation that identifies parameters. Appendix A demonstrates this issue using publicly available data. The main identification source for our parameters of interest comes from counties that are both a *winner* in one or more cases *and* also a loser in at least one case. The next major source of identification comes from differences between incumbent plants in counties that appear with the same status for more than one case. Because the case fixed effects can only be separately identified from the county fixed effects and DD indicators for counties that either appear more than once in the data as either a winner, loser, or both. Removing counties that appear as both a winner and loser changes the number of within-case comparisons and within-county variation identifying the estimates. The remaining identification comes solely from the pooled-time variation for counties that appear for only one case.

It is also important to note that identification requires that for counties that appear for multiple cases, some plants continuously appear in the pre- and post-period data for all cases. These are stringent requirements. Counties have to appear as **both** a winner and loser within the case set and at least one of these counties must plants that continuously appear over the pre- and post-periods for the winning case and the losing case. If not, then the model is not fully identified. Point estimates therefore depend upon the solution to this “dummy variable trap”. Appendix A discusses this issue in detail and provides evidence on the consequences for identification.

Figure 1, Appendix Figures B1-B5, and Appendix Tables B1-B2 present the results of replicating the GHM test of identifying assumptions in their Table 4 and Figure 1 for all the samples of incumbent plants in Table 3. We use the replication code from GHM for this estimation. Each panel presents results for the sample of incumbents associated with each case set and identification strategy. Specifically, we estimate $\ln(Y_{pijt}) = \beta_1 \ln(L_{pijt}) + \beta_2 \ln(K^B_{pijt}) + \beta_3 \ln(K^E_{pijt}) + \beta_4 \ln(M_{pijt}) + \theta_{w\tau} \sum_{\tau=-7}^{5} [1(Winner)_{pj} \times 1(t = \tau)_{jt}] + \theta_{l\tau} \sum_{\tau=-7}^{5} [1(Loser)_{pj} \times 1(t = \tau)_{jt}] + \alpha_p + \mu_{it} + \lambda_j + \varepsilon_{pijt}$ *weighted* by initial value of shipments. The coefficients Column 1 for each panel in the appendix tables presents the estimated difference between $\theta_{w\tau}$ and $\theta_{w,-1}$, and likewise, Column 2 presents the estimated difference between $\theta_{l\tau}$ and $\theta_{l,-1}$. Column 3 is the difference in these differences. The panels in Figures 1a-1e depict the estimated difference in these differences, or $(\theta_{w\tau} - \theta_{w,-1}) - (\theta_{l\tau} - \theta_{l,-1})$, and the 95% confidence interval. Appendix Figures B1-B5 depict the differences for winners and losers separately.

[Inert Figures 1a-1e approximately here]

Figures 1a and 1b present the relative year difference-in-differences for case set 1 winner relative to the revealed ranking and propensity score counterfactuals, respectively, and suggest general downward trend in the pre-period followed by an upward trend in the post-period. These effects are not statistically significant in Figure 1a for the revealed ranking strategy, but winning county establishments tend to have significantly higher TFP relative to the base year in a few of the pre-periods as well as a few of the post-periods compared to the differences in propensity-score loser counties. Appendix Figure B1, Figure B2, and Table B1 reveal that this is driven predominantly by changes in winning counties. Figures 1c-1e demonstrate that the relative-year difference-in-differences are generally statistically insignificant for case-sets 2, 3, and 4, respectively, with no discernable pre-period trends in the differences either. Taken together, these estimates suggest that counterfactual strategies perform reasonably well – although case-set

1 estimates may be less reliable than estimates for the other case sets.

Tables 4 and 5 present the results of estimating equations 1 and 2 using each incumbent-plant sample for our 4 case sets weighted by plant initial output, and unweighted, respectively. The reported mean shift is the equivalent of the θ_1 parameter from estimating equation 1 and the change after 5 years is calculated as $\theta_1 + 6\theta_2$ from estimating equation 2.

Table 4 Column 1 reports the results from our efforts to replicate the main GHM spillover estimates (GHM Table 5 Column 4). It suggests that (continuously-appearing) incumbent plants in winning counties did not experience a statistically-significant mean-productivity shift, but experienced a significant increase of almost 9% at five years. This is driven by breaks in previous productivity trends after the MDP opening relative to losing-county plants. Although Census Bureau restricted-access programs are used to replicate GHM's results, our estimates differ slightly from their 5% mean shift and their 12% effect after 5 years. However, we verify statistically significant increases in winning-county incumbent-plant productivity relative to reported loser-county productivity. We find a similar effect for GHM MDP winning-county incumbents compared to incumbent plants in the nearest propensity-score neighbors. Table 4, Column 2 reports no statistically significant mean shift, but a significant increase at five years of almost 17%, which is larger than using GHM's identification. Again, this is driven entirely by the post-opening trend-break depicted in Figures 1a and 1b.

Table 4 Columns 3-5 indicate that the large change in incumbent plant productivity after five years may be unique to the particular set of GHM MDPs. Recall that case-set 2 includes the GHM MDPs as well as a subset of any MDPs reported by the magazine that weren't in the GHM sample, as well as the highly-incentivized large-plant locations reported in the Good Jobs First Database. Despite having, on average, output that is a larger share of winning-county output than GHM MDPs, Table 4 Column 3 indicates that this MDP set did not generate significant productivity spillovers for continuously-appearing incumbent plants. Similarly, there is no significant productivity effect associated with randomly-drawn large new plant births (case-set 4 results in Column 5).

[Insert Table 4 approximately here]

In Table 4 Column 4, we find a more moderate 2% increase in productivity for the average plant in case-set 3 MDP winning counties after 5 years. Recall that case-set 3 plants include both GHM MDPs that appear in the magazine as well as a subset of *Site Selection* magazine and Good Jobs First MDPs through 1997. These MDPs represent a smaller share of winning-county output than GHM MDPs with few employees but much larger payrolls. Case-set 2 plants are a subset of case 3 plants, with the difference being plants associated with MDP cases from 1994-1997. Thus, the positive productivity shock could result from changing dynamics associated with incentives over time or higher human capital in new plants. That the productivity gains are small relative to the GHM winners may be attributable to upward wage pressure in winning counties after the MDP opening, incumbent firms losing human capital to new entrants, or other potential congestion forces.

Table 5 repeats the estimations from Table 4 without weighting plants by their initial-shipment value. These estimates may be thought of as estimates for the average case rather than average plant. In all cases, the significant changes in winning-county productivity disappear. This suggests that Table 4's results may be driven by a few (of the already select sample) of continuously-appearing incumbent plants and that the average MDP does not generate significant positive productivity spillovers. Further, recall that most of the identification comes from a small subset of counties for multiple cases (as well as continuously appearing firms in the sample).

[Insert Table 5 approximately here]

Although the choice of counterfactuals ideally generates treatment assignment that is as good as random, there are still potential endogeneity sources that may present concerns. In particular, incumbent plant output, capital expenditure, labor, and material inputs are simultaneously determined by the firm and these decisions may also be affected by time-varying unobservables that affect both selection and incumbent-plant TFP. We follow the best practices outlined in Combes and Gobillon (2015) and estimate the spillover effects using a two-step procedure. We use a variant of the Combes et al. (2008, 2010) two-stage estimator adapted to a production function and our context. Our preferred two-step method directly addresses the simultaneity of the output and inputs as well as firm heterogeneity using the Levinsohn-Petrin (2003) estimator in the first stage.

3.4. Two-stage-fixed effect estimates

In our variation of the Combes et al. (2008, 2010) two-stage procedure, we first estimate

$$(3)\ \ln\left(Y_{pijt}\right) = \beta_1 \ln\left(L_{pijt}\right) + \beta_2 \ln\left(K^B_{pijt}\right) + \beta_3 \ln\left(K^E_{pijt}\right) + \beta_4 \ln\left(M_{pijt}\right) + B_{c(j)it} + \varepsilon_{pijt},$$

where $B_{c(j)it}$ is a vector county-2 digit SIC code industry-time fixed effects. The first-stage estimates a production function weighted by the initial value of output and county-industry-year fixed effects. Note that equation (3) does not include a plant fixed effect as we cannot separately identify $B_{c(j)it}$with plant fixed effects included.

We estimate the spillover effect in the second-stage with:

$$(4)\ \hat{B}_{c(j)it} = \delta 1(Winner)_{cj} + \kappa 1(\tau \geq 0)_{jt} + \theta_1\left[1(Winner)_{cj} \times 1(\tau \geq 0)_{jt}\right] + \mu_{it} + \lambda_j + \epsilon_{cjit}, \text{ and}$$

$$(5)\ \hat{B}_{c(j)it} = \delta 1(Winner)_{pj} + \psi Trend_{jt} + \Omega\left[Trend_{jt} \times 1(Winner)_{pj}\right] + \kappa 1(\tau \geq 0)_{jt} + \gamma\left[Trend_{jt} \times 1(\tau \geq 0)_{jt}\right] + \theta_1\left[1(Winner)_{pj} \times 1(\tau \geq 0)_{jt}\right] + \theta_2\left[Trend_{jt} \times 1(Winner)_{pj} \times 1(\tau \geq 0)_{jt}\right] + \mu_{it} + \lambda_j + \epsilon_{cjit}.$$

This strategy has benefits of the two-stage estimator outlined by Combes et al., but also has the added benefit of identifying our key parameters from a larger set of plants. The key parameters are still identified by plants in counties that appear for more than one case. However, identification now comes from all plants in those counties that continuously-appear for at least one case rather than only from plants that continuously-appear for all cases.

The second-stage estimates from our variant of the Combes et al. (2008, 2010) two-step procedure outlined in equations 3-5 are presented in Table 6. The estimates in Table 6 continue to indicate significant increases in productivity after 5 years associated with the GHM winner counties set 1. The revealed ranking estimate of 8.7% (Column 1) is virtually unchanged from the weighted estimate in Table 4; however, the estimated spillover identified by the propensity score losers is cut nearly in half to 8.9% (Column 2). Table 6 indicates no significant productivity spillovers for incumbent plants associated with the other 3 case sets.

[Insert Table 6 approximately here]

3.5. Two-stage Levinsohn-Petrin estimates

Our preferred variant of the two-stage procedure allows us to explicitly address the potential endogeneity of inputs, reintroduce the plant fixed effect, and examine the full distribution of sample plants' residual (log) TFP in the first-stage. Specifically, we first estimate

$$(6) \qquad \ln\left(Y_{p\tilde{\imath}jt}\right) = \beta_1 \ln\left(L_{p\tilde{\imath}jt}\right) + \beta_2 \ln\left(K^B_{p\tilde{\imath}jt}\right) + \beta_3 \ln\left(K^E_{p\tilde{\imath}jt}\right) + \beta_4 \ln\left(M_{p\tilde{\imath}jt}\right) + \alpha_p + \varepsilon_{p\tilde{\imath}jt}$$

using the Levinsohn-Petrin (2003) estimator. We then predict each plant's residual and use a Gaussian kernel to estimate the distribution of TFP by winner status before and after the large plant opening. We then average residual TFP by county, 3-digit SIC code industry, and year to get $\hat{\bar{\varepsilon}}_{c(j)\tilde{\imath}t}$. The second-stage estimates the spillover effects using the predicted average residual TFP in each county-industry-year with

$$(7) \; \hat{\bar{\varepsilon}}_{c(j)\tilde{\imath}t} = \delta 1(Winner)_{cj} + \kappa 1(\tau \geq 0)_{jt} + \theta_1\left[1(Winner)_{cj} \times 1(\tau \geq 0)_{jt}\right] + \mu_{it} + \lambda_j + \epsilon_{cj\tilde{\imath}t}, \text{ and}$$

$$(8) \; \hat{\bar{\varepsilon}}_{c(j)\tilde{\imath}t} = \delta 1(Winner)_{pj} + \psi Trend_{jt} + \Omega\left[Trend_{jt} \times 1(Winner)_{pj}\right] + \kappa 1(\tau \geq 0)_{jt} + \gamma\left[Trend_{jt} \times 1(\tau \geq 0)_{jt}\right] + \theta_1\left[1(Winner)_{pj} \times 1(\tau \geq 0)_{jt}\right] + \theta_2\left[Trend_{jt} \times 1(Winner)_{pj} \times 1(\tau \geq 0)_{jt}\right] + \mu_{it} + \lambda_j + \epsilon_{cj\tilde{\imath}t}.$$

Before proceeding to spillover estimates, it is useful to examine all plants' (unaveraged) residual (log) TFP from a Levinsohn-Petrin estimator with plant and industry-year fixed effects. Figures 2-6 present the kernel density estimated distribution of plant residual TFPs by winner status before and after MDP openings. The second-stage estimates are still primarily identified by mean-residual TFP in counties that appear more than once, but distributions in Figures 2-6 use information on all (continuously-appearing) incumbent plants in all winner and loser counties.

Panel a of Figure 2 depicts the distribution for GHM winner and loser incumbent TFP before the MDP opening. The distributions are very similar except for the tails where there is little mass. The lowest productivity incumbents in loser counties have much lower residual TFP than those in winner counties. In the other tail, there are higher-productivity incumbents in winner counties than losers. The most notable change after the MDP opening (Panel b) is tighter distributions for both winner and loser incumbents, with much more mass around the mean and much shorter tails. It is also noteworthy that the lowest-productivity plant(s) are now in winner counties and the highest productivity plants are in loser counties. Figure 2 suggests that the plants identifying the large estimated changes in GHM winner productivity may not be representative of the relative differences in average winner and loser plant productivity after a large-plant shock.

[Insert Figure 2 approximately here]

Figure 3 contains the before and after distributions of residual TFPs for GHM winners compared to their nearest propensity-score losers. Again, the winner and loser distributions are quite similar, with the differences being in the tails. Comparing the distributions before and after reveals slightly less mass at the mean and a slightly longer low-productivity tail and shorter high-productivity tail for winner counties. This may seem at odds with large, significant productivity spillovers estimated above. However, it is not at odds in other ways – it is entirely possible for plants identifying those estimates to move in the distribution of winner plants.

[Insert Figure 3 approximately here]

The estimated distributions for case-sets 2-4 are presented in Figures 4-6, respectively.

Again, winners and losers generally have similar distributions within each case set. There are some particularly low-productivity incumbents in loser counties for case-set 2, as well as similar high-productivity winner and loser incumbents, resulting in less mass at the mean for losers in case set 2. This difference disappears after the MDP opening. Interestingly, there also appear to be more gains in the high productivity tail in winning counties than in losing counties. The opposite is true for case set 3 after the MDP opening and the gains are similar for the highest productivity winners and losers in case set 4.

[Insert Figures 4-6 approximately here]

Table 7 presents the second-stage estimates, where average county-3 digit industry-year residual TFP is regressed on our parameters of interest, 2-digit industry-year and case fixed effects. Consistent with the depictions in Figures 2-6, there is no statistically significant spillover associated with any set of MDPs.

[Insert Table 7 approximately here]

3.6. Two-stage LP estimates using LQ counterfactuals

The geographically-proximate propensity-score matching strategy relies on observables; yet, unobservable locational advantages may also play a key role. We therefore repeat our preferred spillover estimator using counterfactuals that are the nearest MDP-industry location-quotient neighbors within 100-250 miles of the winner counties. Appendix B Table B1 contains summary statistics for the resulting incumbent-plant samples. It reveals that the location-quotient strategy produces a sample of loser incumbents that is very similar to winners in terms of output and labor; however, the losers tend to have fewer incumbent plants in total. Appendix Figures B1-B4 present the estimates from an event-study specification of the second-stage equation (7). It is clear from the figures that our identifying assumptions are strongly satisfied, with very similar residual TFP pretrends for winner- and loser-county incumbents and no statistically significant differences.

Table 8 presents the mean shift and change after 5 years estimated using the location-quotient counterfactuals in the second–stage equations 7 and 8. The results are very similar to those in Table 7, with no statistically significant changes in incumbent-plant productivity in winning counties after the MDP opening.

[Insert Table 8 approximately here]

4. Non-parametric estimates of the agglomeration function

Having established the size of spillovers associated with MDPs (or lack thereof), we now consider how estimated TFP changes with employment density in economically-close plants in the county. This also allows us to assess whether our preferred estimation strategy detects the agglomeration externalities that we expect given many interactions.

We nonparametrically estimate the link between local plant density on plant output in a partially linear-regression model using Robinson's (1988) double residual semiparametric regression estimator.[11] We consider the following model of the log of plant output

$$(9) \qquad \ln\left(Y_{p\tilde{\imath}jt}\right) = \beta_1 \ln\left(L_{p\tilde{\imath}jt}\right) + \beta_2 \ln\left(K^B_{p\tilde{\imath}jt}\right) + \beta_3 \ln\left(K^E_{p\tilde{\imath}jt}\right) + \beta_4 \ln\left(M_{p\tilde{\imath}jt}\right) + \alpha_p + \mu_{it} + \varepsilon_{p\tilde{\imath}jt}$$

estimated using the Levinsohn-Petrin (2003) estimator. Similar to the steps before, we then predict the residual for each plant and average them by county, 3-digit SIC code industry, and year to derive $\hat{\bar{\varepsilon}}_{c(j)\tilde{\imath}t}$. We then semiparametrically estimate spillover effects and the agglomeration function in the second-stage estimates using the predicted average residual TFP in each county-industry-year:

$$(10) \qquad \hat{\bar{\varepsilon}}_{c(j)\tilde{\imath}t} = \delta 1(Winner)_{cj} + \kappa 1(\tau \geq 0)_{jt} + \theta_1\left[1(Winner)_{cj} \times 1(\tau \geq 0)_{jt}\right] + \lambda_j + g\left(ln\left(\frac{E_{i(p),j,c(j),t-s}}{R_{c(j)}}\right)\right)\epsilon_{cj\tilde{\imath}t},$$

[11] Robinson's (1988) double residual semiparametric regression estimator is implemented in Stata with semipar and is more stable than Yatchew's semi-parametric difference estimator (see Verardi and Debarsy 2012).

where $g\left(ln\left(\frac{E_{i(p),j,c(j),t-s}}{R_{c(j)}}\right)\right)$ is an unknown function of log (weighted) number of employees *E* per-square mile in plant *p*'s county *c* for case *j*, in year *t-s*. Local plant density is defined for each incumbent-plants' industry-county-year combination, with weights determined by economic "closeness" between plant *p* and the establishment in which employees work. This is therefore an estimate of the agglomeration-function's shape. It tells us how TFP varies with the density of economically-close plants regardless of the relationships with the MDP. Lagging local-plant density makes the model deterministic and prevents plants from producing wildly different output levels in any period purely by chance (Alexander and Ray 1998; Kline and Moretti 2014). The θ_1 and δ coefficients are identified as before. Defining g(·) in this way is comparable in spirit to Kline and Moretti (2014).

We consider four definitions of local-plant density in which weights are determined by economic "closeness." The first definition considers only own-industry density for *p*. The other three use supplier, customer, and labor linkages outlined in GHM to measure "closeness." Industries are defined by their three-digit SIC codes.[12] Own-industry plants are therefore establishments which belong to the same SIC code as the MDP. Supplier and customer industries are defined using the Bureau of Economic Analysis 1987 Input-Output Accounts. The similarity of shared labor inputs is determined as in Ellison et al. (2010), using the Bureau of Labor Statistic's 1987 National Industrial-Occupation Employment Matrix (NIOEM). Similarity is determined through correlations of the fraction of each industry's employment in each occupation in the NIOEM.

Table 9 reports the mean and standard deviation of weighted employment density from equation 10, where the number of employees *E* per square mile in plant *p*'s county *c* for case *j* in year *t-1* is weighted by the economic distance between plants' 3-digit SIC industries, for each

[12] The post-2001 data contain 2007 and 2002 NAICS codes rather than SIC codes. We follow the MDP plants through time and assign the SIC codes from their earlier entries to their later entries. In order to calculate county-industry share variables, we convert sample county plant NAICS codes to SIC codes. We use the NAICS-SIC crosswalks available on the RDC servers to convert the NAICS codes to SIC codes.

definition of economic closeness and case set by winner status and treatment period.[13] Plant employment is calculated from the Longitudinal Business Database and includes employment from all plants in the county-year-industry rather than just continuously-appearing incumbents.

[Insert Table 9 approximately here]

Panel A contains weighted densities for the GHM winners and losers. It is interesting to note that all four measures of economically-close plant densities are higher for losing incumbents than winning incumbents prior to the large plant opening (Columns 2 and 3). Yet, this difference shrinks in the post-period due to an increase in economically-close plant densities for winning establishments and a decrease for losing establishments. Such coincident changes in underlying plant densities and the subsequent effect on incumbent-plant productivity is captured in the above spillover estimates that don't directly control for changes in densities. To the extent that these employment-density changes are attributable to the MDP, then it is reasonable to consider these as part of the MDP effect. To the extent that they are attributable to other location-specific attributes (perhaps those that also attracted the MDP), then the above estimates overstate the spillover effects attributable to the MDP from the change in productivity attributable to the change in underlying economically-close plant densities.

Examining Table 9 Panels B-E, we do not find such systematic differences by treatment status and period. In general, economically-close employment-density in winners and losers is less in the post-period than in the pre-period. This is consistent with increased automation decreasing manufacturing employment during the study period. It is also noteworthy that, with the exception of case-set 1, the pretreatment mean densities are quite similar for winner and loser incumbents.

Consistent estimation of Equation (10) requires that lagged density is uncorrelated with contemporaneous, unobserved shocks. While it is possible this assumption holds, we address this possible source of endogeneity by instrumenting for lagged density. We use a deep-lag of

[13] All MDP plants and plants owned by the MDP firm are excluded from the employment density calculations.

historic market potential from 1940 as our instrument. The simple market potential measure is based upon population and income outside the county of interest and is a measure of the potential size of a firm's market. As such, it is a good predictor of plant density due to historic transport costs and past firm location decisions, but uncorrelated with contemporaneous productivity shocks within the county. In practice, we constructed our instrument as the inverse-distance-squared, weighted sum of 1940 income for all counties within 500 miles of a given county.

Figures 7-11 contain the semiparametric fit of mean county-industry-year residual TFP as a function of our four measures of economically-close plant employment density obtained by estimating equation 10 for the GHM winners and losers (Figure 7) and case-sets 1-4 using propensity-score losers (Figures 8-11, respectively). The fitted observations in Figures 7-11 can be interpreted as the shape of the agglomeration function with respect to each definition of economic proximity.

Figure 7 suggests some non-linearities in the agglomeration function. The semiparametric fit of residual TFP with respect to own-industry and manufacturing-output customers both increase steadily until peaking at slightly over 100, and then decline. The mean (standard deviation) of own-industry and manufacturing-output customer density in this sample is 2.645 (11.05) and 2.34 (10.78), suggesting that congestion forces begin to dominate agglomeration externalities at densities many standard deviations above the mean. The relationship between manufacturing-input-supplier density and TFP in Figure 7 also suggests a potential non-linearity, with an approximately linear increase until around 30, at which point the slope sharply increases. The productivity increase associated with greater density of skilled employees, however, appears approximately linear.

[Insert Figure 7 approximately here]

Figure 8 depicts the semiparametric fit for case-set 1's (GHM) winners and propensity-score losers. Figure 8 reveals a steady increase in county-industry-year mean residual TFP as all four measures of economically-close plant densities increase. Further, all four relationships appear approximately linear, with slightly steeper slopes associated with own-industry and manufacturing-output customer densities. There is no evidence of a turning point at which congestion externalities begin to dominate agglomeration externalities associated with own-industry and manufacturing-output customer density – likely because this sample does not contain county-industry-years for which own-industry and manufacturing-output customer densities exceed 100. Similarly, we do not observe a sharp increase in the manufacturing-input-supplier density slope because we don't observe any plants exceeding a density of 30.

[Insert Figure 8 approximately here]

The shape of the agglomeration functions estimated from case-set 2 are illustrated in Figure 9. The relationships generally appear linear, except for the sharp increase in slope associated with manufacturing-input-supplier density around 30. This is the same relationship observed in Figure 7 and similarly represents a density several standard deviations higher than the sample mean.

[Insert Figure 9 approximately here]

Figure 10 contains the estimated relationship between residual TFP and economically-close employment densities for case-set 3. The increase in the slope associated with manufacturing-input-supplier density around 30 also appears in Figure 10, although the estimates at such high densities are noisy. Similar to Figure 8, the TFP gains from increases in manufacturing-output customers peak around 100 and then decline before recovering. The increases in TFP associated with increased own-industry and share of skilled employees appear approximately linear.

[Insert Figure 10 approximately here]

On the other hand, the only indications of nonlinearity for case-set 4 in Figure 11 come from the estimated residual-TFP changes as a function of own-industry and skilled-worker-share densities. Rather than peaking at an own-industry density of 100 like Figure 7, Figure 11 indicates a sharp increase in slope that peaks at just over 200 before declining to become negative. Agglomeration externalities appear to dominate congestion externalities again around 450 – a level of own-industry density unobserved in previous samples. Congestion effects overtake agglomeration benefits again at a density of 550, with steady declines in residual TFP as own-industry densities increase beyond that point. The nonlinearity associated with shared labor density also appears only at densities unobserved in other samples. There is a linear increase in residual TFP from greater shared labor densities until around 1000, where there is a sharp increase in slope that peaks just before 2000, before steadily declining.

[Insert Figure 11 here]

Taken together, Figures 7-11 suggest that we cannot reject linear agglomeration functions over the range of densities observed for most plants. Nonlinearities in the agglomeration functions appear at very high densities – around own-industry and manufacturing-output-customer densities of 100, manufacturing-input-supplier densities around 30, and shared-labor densities of 1000.

5. Lasting effects

Although Section 3 suggests weaker than expected spillovers for incumbent plants, Section 4 establishes that there are positive agglomeration externalities for these plants that are consistent with models featuring long-lasting effects from shocks, but at ranges of “agglomeration” densities far above the range that is commonly observed in local economies. We therefore turn to the question of long-run effects by estimating the persistence of the MDP output shocks with respect to the location’s place in the distribution of U.S. manufacturing activity. This analysis uses all plants in the county and therefore includes new entrants, deaths, and smaller establishments that are excluded in previous sections. In the appendix, we take extend this analysis to a multiple-equilibria setting. Finally, we look specifically at the twenty-year effects on new establishment births and deaths.

5.1. Persistence Methodology

We examine the change in county-manufacturing and county-manufacturing-industry shares of national manufacturing and manufacturing-industry after 20 years using the methodologies developed in Davis and Weinstein (2002; 2008) and Bosker et al. (2007). We define county-manufacturing (log) share at time τ in county c receiving MDP shock j as its location quotient $s_{c(j)\tau} \equiv \ln(S_{c(j)\tau}) = \ln\left(\frac{manufacturing\ output_{c(j)\tau}}{manufacturing\ output_{US\tau}}\right)$.[14] Assuming a unique, stable equilibrium, county manufacturing shares at time τ can be modeled as $s_{c(j)\tau} = \Pi_{c(j)} + \varepsilon_{c(j)\tau}$, where $\Pi_{\mathrm{c(j)}}$ is the initial equilibrium size in county c and $\varepsilon_{c(j)\tau}$ is a location-specific shock to a local areas'manufacturing share. Persistence of shocks takes the form $\varepsilon_{c(j),\tau+1} = \rho\varepsilon_{c(j),\tau} + \nu_{c(j),\tau+1}$, where $\rho \in [0,1)$ is the persistence parameter.

Let $\nu_{c(j),5}$ be the MDP shock to county output during the first-five years after opening and $\nu_{c(j),20}$ be the typical idiosyncratic location-specific shock to manufacturing share around the new post-MDP equilibrium $s_{c(j),20}$.[15]As shown in Davis and Weinstein (2002), our equation for the effect of the MDP shock to winning-county c(j)'s share of manufacturing output becomes:

$$(11) \quad s_{c(j),20} - s_{c(j),5} = (\rho - 1)\nu_{c(j),5} + \left[\nu_{c(j),20} + \rho(1-\rho)\varepsilon_{c(j),-5}\right],$$[16]

where the term in brackets is the error term, which is assumed uncorrelated with the MDP shock. We use Census microdata on the large firm locations and therefore know the size of the MDP shock. We define the MDP shock as $MDPshock = \max\left(MDP\ output_{c(j)\tau}\right), \tau \in (0,5)$, or the maximum amount of output reported by an MDP-owned firm in the winning county from the announcement date through the first-five-full years of operation, by which time we expect the MDP to be at its stable size. We then define the shock to winning county output shares as $\nu_{c(j),5} \equiv MDPshock/output_{c(j),-1}$, which we can relate to the growth rate in the winning-county manufacturing shares during this period by noting $s_{c(j),5} - s_{c(j),-1} \approx \left(\frac{\frac{mfg\ output_{c(j),-1} + MDP\ output_{c(j),5}}{mfg\ output_{US,-1}} - \frac{mfg\ output_{c(j),-1}}{mfg\ output_{US,-1}}}{\frac{mfg\ output_{c(j),-1}}{mfg\ output_{US,-1}}}\right) = \frac{MDP\ output_{c(j),-1}}{mfg\ output_{c(j),-1}}$.

The persistence test rests on the value of ρ. If $\rho = 1$, then the shock is permanent and shares follow a random walk (i.e., the shock leads to a new equilibrium). If $\rho = 0$, then the shock

[14] We measure the location's manufacturing-output share from the data using all reporting plants.
[15] Using the first-five years after the opening of an MDP plant to make an assessment of its effects follows GHM.
[16] As suggested in Bosker et al. (2007), we use growth rates constructed as $\frac{x_t - x_{t-i}}{x_{t-i}}$, rather than $\ln(x_t) - \ln(x_{t-i})$ because the latter is only a valid measure of growth for small changes.

dissipates fully. $0 \leq \rho < 1$ suggests a mean-reverting process and we can reject manufacturing shares following a random walk. This may or may not be consistent with multiple equilibria.

It is possible that $\rho \neq 0$ because of some correlation between *future* changes in county-manufacturing shares and *past* changes that we do not model. Thus, we include pre-MDP opening growth in the county's manufacturing share as a control in the estimating equation. It is also possible that MDP shocks are correlated with the error term and we cannot obtain a consistent estimate of ρ by directly estimating (10). Instead, we instrument for the MDP shock using average national-establishment output for firms in the MDP's 4-digit SIC industry and average national new-entrant output in the MDP's 3-digit industry in time $\tau = -1$, expressed as a share of the initial winning county's manufacturing output. We use instruments from $\tau = -1$ to avoid any concerns that the MDP's output decision or announcement influence other firms' output decisions.[17] Our estimating equation therefore becomes:

$$(12) \quad s_{c(j),20} - s_{c(j),5} = \alpha \hat{v}_{c(j),5} + \beta_0 + \zeta PreMDP_{c(j)} + error_{c(j)}{}^{18},$$

where $\alpha = \rho - 1$ and $PreMDP_{c(j)}$is the pre-MDP opening growth in county manufacturing share. Under the null of hypothesis of a unique equilibrium, the coefficient on the instrumented MDP shock ($\hat{v}_{c(j),5}$) equals minus one.

Equation (12) requires a comparison of winning-county shares of national-manufacturing output over time. We construct these shares from the restricted-access Census of Manufacturers (CM). We remove any plants owned by the MDP firms from the micro data and then aggregate the microdata by county-year and county-industry-year. We require years that are not CM years and therefore linearly interpolate between CM years for each county and the nation as a whole. We do not construct shares from ASM data for these years because the ASM sampling scheme

[17] In the spirit of Bosker et al. (2007), we considered a geographical extension and used the distance-weighted change in surrounding county manufacturing shares as an instrument; however, conditional on our other instruments and controls, this had little power in the first-stage and we therefore report estimates using MDP industry-output instruments.
[18] We include a constant to allow for the possibility that the error is not mean zero, i.e., shares do not add to one.

described above could introduce errant variation in shares.

5.2. Persistence and Multiple Equilibria Results

Tables 10 and 11 presents the results of instrumental-variable estimation of equation (12) for the persistence parameter of the MDP shock. The tables also report tests of the null of a unique equilibrium by testing whether the coefficient on the MDP shock is minus unity. Table 10 examines winning-county manufacturing shares and Table 11 considers winning-county manufacturing shares by industry, with columns representing case-sets 1, 2, 3, and 4, respectively. Appendix Tables D1-D8 report the multiple equilibria analyses.

Table 10 presents the manufacturing share results. Column 1 reports the results for case-set 1's winning counties, the counties receiving the GHM MDP shocks. Recall that this was the only case set for which we estimated positive spillovers and therefore the most likely candidate for persistent effects. We can reject that the coefficient on the MDP shock is minus unity in the IV specification reported in Table 10 Column 1. In other words, we can reject that $\rho = 0$ and that the shock fully dissipates. Yet, with $\alpha = \rho - 1$ statistically indistinguishable from zero, the shock appears highly persistent. As Davis and Weinstein describe, we can also interpret this as a rejection of the null of a unique equilibrium. However, Appendix Table D1 places a unique equilibrium within the same framework as multiple equilibria and the data most strongly support a unique equilibrium despite the IV model's rejection of the unique-equilibrium null.

[Insert Table 10 approximately here]

Table 10 Column 2 presents the results for county winners from case-set 2 MDPs, which represented the largest share of winning-county output, on average, in our samples of MDP cases. Here, we can also reject the null hypothesis that the coefficient on the MDP shock is minus unity. Again, this suggests that the shock is quite persistent. It also suggests that we can reject the null hypothesis of one unique equilibrium, the threshold regressions in Appendix Table D2 suggest that the unique-equilibrium specification better describes the data than any of the multiple equilibria specifications.

Table10 Columns 2 and 3 present the results for case-sets 3 and 4, respectively. In both cases, the IV specification indicates that we can reject the null of a unique equilibria, but the Schwarz criterion test of multiple-equilibria suggests these case sets are unique-equilibria specification. The rejection of the coefficient on the MDP shock as equal to minus unity in the IV specifications suggests that $\rho \neq 0$, which is the assumption imposed by our multiple equilibria tests. However, the estimated coefficients are positive suggesting that $\rho > 0$. As discussed in Davis and Weinstein (2008), $\rho > 0$ would work to bias our multiple-equilibria tests in favor of finding multiple equilibria. Even so, the fact that the data are still best described by the unique-equilibrium specification provides even stronger evidence in support of a unique equilibrium. Although the results do not indicate that these shocks are large enough to push the location to a new-equilibrium manufacturing share, the IV estimates do indicate that the shocks are persistent and push winning-county manufacturing output higher for a "lengthy" period.

Table 11 reports analogous estimates for county MDP-*industry* shares rather than *overall* manufacturing shares. Now we are instead assessing whether multiple equilibrium applies more narrowly to the MDP's industry rather than for overall manufacturing. The sample of winning counties decreases for this case because it is limited to those winning counties that had pre-existing establishments in the MDPs own 3-digit SIC industry. We reject the null of a $\rho = 0$ for all 4 case sets. Yet, the data still most strongly supports a unique equilibrium when analyzed within the same framework as multiple equilibria for case-sets 1-3 (Appendix Tables D5-D7). Interestingly, the data most strongly support multiple equilibria for case-set 4 (Table D8), with the two-equilibria specification maximizing the Schwarz criterion and passing the intercept ordering criterion that requires a larger positive shock be associated with larger, new equilibrium share and that the thresholds lie between equilibrium shares.

[Insert Table 11 here]

Recall that case-set 4 is the set of randomly drawn largest employers in the micro data – not a set of highly-incentivized large plants. These large plant shocks had, on average, much smaller output and employment shocks than the highly-incentivized-plant sets. However, they had much larger payroll shocks, potentially suggesting greater shares of highly-skilled employees and higher labor productivity. Thus, it is possible that it is *the size of the human capital shock*, rather than *the size of the output shock*, that matters most for pushing a location to a new-equilibrium share of the manufacturing-industry's output. The fact that the data does not support multiple equilibria for case-set 3 shocks, which also have at least some high human capital MDPs, casts some doubt on this. Instead, it may be that multiple equilibria are supported precisely because the case-set 4 shocks are not highly-incentivized and thus forego some of the general equilibrium congestion costs, crowd-out effects, and moral hazard effects associated with incentives.

5.3. Births and Deaths

In order to better understand the dynamics associated with our persistence findings, we examine manufacturing-establishment births and deaths in our winner and loser counties. We use the restricted-access Longitudinal Business Database (LBD) to identify new, manufacturing-establishment births and deaths in those counties. Figures 12-15 depict the mean (log) number of births and deaths in winner and loser counties by year relative to the MDP opening.

Figure 12 reveals GHM winner counties have fewer manufacturing births and deaths, on average, than GHM loser counties. These results are consistent with Partridge, Schreiner, Tsvetkova, and Patrick's (2020) findings that greater overall intensity in tax incentives aimed at (mainly large) firm attraction had rather large crowding-out effects on business startups. Figure 12 indicates that winner and loser birth trends are approximately parallel after the MDP opening, with, perhaps, a slightly slower decline in winning counties. On the other hand, the pre-opening death trends in winner and loser counties are remarkably parallel and winner counties appear to experience a sharp uptick in establishment-death rates after the MDP opening. This is consistent with a creative destruction process for less-productive firms, as well as congestion costs related to large plant shocks and incentives. It also suggests that surviving plants that continuously-appear in the data are likely higher-productivity plants, meaning that some of the relative gains in winner counties may be a *composition effect* from the death of lower-productivity plants.

GHM winner counties also experienced fewer births and deaths, on average, than their propensity-score counterfactual counties – although the difference is smaller. Figure 13 suggests slight differences in pretrends and fairly parallel trends after the MDP-opening.

[Insert Figures 12-16 approximately here]

Figures 14-16 indicate that winning counties experienced more births and deaths, on average, than their propensity-score neighbors. The figures generally suggest similar pretrends and birth trends after the MDP opening. Although not as dramatic as with the GHM sample, there is some indication of a slight increase in the establishment-death rates in winning counties after the opening, which again likely increases overall county productivity, but offsets job growth.

To provide a more formal test of MDP-induced changes in county births and deaths, we estimate $\ln(births)_{cjt} = \delta 1(Winner)_{cj} + \kappa 1(\tau \geq 0)_{jt} + \theta_1\left[1(Winner)_{cj} \times 1(\tau \geq 0)_{jt}\right] + \lambda_j + \epsilon_{cjt}$ for the five years prior and twenty years after the MDP opens. We estimate an analogous equation for establishment deaths.

Table 12 presents the birth specification estimates. The results paint a uniformly bleak picture with regards to large new plant openings and establishment births, i.e., successfully luring an MDP is associated with either no increase or a significant decrease in establishment births. Although imprecisely measured, Columns (1) –(3) suggest either an economically small increase or relatively large decrease in new manufacturing-establishment births after an MDP opens. Columns (4) and (5) indicate a decrease of 6 and 9 percent, respectively. This is consistent with the idea of crowding-out effects associated with these new MDPs and inconsistent with the notion that they induce significant new economic activity.

There is mixed evidence regarding death rates in Table 13. Columns (1) and (2) indicate an imprecisely measured increase in establishment deaths in GHM MDP winning counties. Again, this supports the idea of MDP- or incentive-induced costs borne by existing establishments. Columns (3)-(5), on the other hand, suggest fewer establishment deaths – albeit, economically small and imprecise for case-sets 2 and 3. Column 5 reports a significant 5 percent decline in deaths in counties that attracted one of the *random new, large manufacturing establishments*. However, this reduction in deaths does not completely offset the decline in births.

Taken together, the evidence in this section clearly illustrates how successful attraction of one highly-incentivized large new facility fails to have "positive" long-run effects through indirectly inducing births or foregoing deaths.

6. Conclusions

We verify significant spillovers from GHM MDP openings, leading to five-year cumulative changes in incumbent-plant productivity, albeit without the large mean-shift GHM found. However, these spillovers are driven by a unique set of plants that operate continuously in a county. These plants also appear for ***more than one*** *winning and/or losing case.* The results from this unique set of MDP openings are *not* robust to alternative identification strategies. Our results further suggest much weaker spillovers associated with other highly-incentivized MDP openings, but again, these effects are identified from a unique set of plants driven by the empirical strategy. These findings are consistent with ideas that agglomeration externalities are a function of many interactions and adding one establishment (even very large ones) is unlikely to produce large changes in overall productivity levels. We do, however, find that TFP rises as interactions with economically-close plants increase. This is consistent with agglomeration-externality models possessing persistent effects from shocks and multiple equilibria.

We find some evidence that GHM MDP shocks have persistent, positive effects on winning-county manufacturing shares and manufacturing output. While we cannot rule out multiple equilibria, the data generally support one unique, equilibrium county manufacturing share. Thus, our results suggest that even in the presence of significant spillovers for some plants, large-plant openings are not sufficiently large positive shocks to push locations into a new equilibrium. We also find no evidence that the persistent output effects of the MDP shocks are driven by MDP-induced births or deaths. If anything, MDPs appear to induce fewer births in winning counties that are not offset by reductions in deaths. This may be due to countervailing congestion forces or weaker than anticipated spillovers, or both. Yet, the loss of innovation and jobs from fewer net-startups offset at least some of the supposed gains from MDP attraction.

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FOR ONLINE PUBLICATION

Identifying Agglomeration Spillovers: New Evidence from Large Plant Openings

Appendix

Carlianne Patrick[1]

Mark Partridge[2]

A. GHM Specification and Counterfactual Identification Demonstration

To demonstrate the identification issues raised by the specification and revealed rankings counterfactuals, I obtain total county level employment data from the County Business Patterns and match it to the full set of manufacturing winner and loser counties in for 64 cases. I summarize the specifications and results below.[3]

As can be gleaned from Table A1, 59 counties in the data appear as a winner for at least one of the 64 cases, 82 counties appear as losers, and six counties win at least one MDP competition and loser at least one other. Table A2 reveals that 4 counties win more than one sample case and 11 counties are losers in two cases.

Table A1: Summary of Counties and Winner Status

	Loser		Total
Winner	0	1	
0	0	76	76
1	53	6	59
Total	53	82	135

[1] Associate Professor, Department of Economics, Georgia State University, cpatrick@gsu.edu
[2] Professor, Swank Chair in Urban Rural Policy, Department of AED Economics, The Ohio State University, partridge.27@osu.edu

[3] To replicate the analysis summarized below, the data, Stata code, and log files may be accessed here.

Table A2: Frequency of Counties' Winner Status

	Frequency	Percent
Winner		
0	78	56.30
1	55	40.74
2	4	2.96
Total	135	
Loser		
0	53	39.26
1	71	52.59
2	11	135
Total	135	

The inclusion of counties that appear as both a winner and loser as well as counties with the same status more than once is important for identification in a difference-in-differences specification with both county and case fixed effects. To see this, recall the GHM plant specifications specification in Equations (1) and (2):

$$(1)\ \ln(Y_{pijt}) = \beta_1 \ln(L_{pijt}) + \beta_2 \ln(K^B_{pijt}) + \beta_3 \ln(K^E_{pijt}) + \beta_4 \ln(M_{pijt}) + \delta 1(Winner)_{pj} + \kappa 1(\tau \geq 0)_{jt} + \theta_1[1(Winner)_{pj} \times 1(\tau \geq 0)_{jt}] + \alpha_p + \mu_{it} + \lambda_j + \varepsilon_{pijt}$$

$$(2)\ \ln(Y_{pijt}) = \beta_1 \ln(L_{pijt}) + \beta_2 \ln(K^B_{pijt}) + \beta_3 \ln(K^E_{pijt}) + \beta_4 \ln(M_{pijt}) + \delta 1(Winner)_{pj} + \psi Trend_{jt} + \Omega[Trend_{jt} \times 1(Winner)_{pj}] + \kappa 1(\tau \geq 0)_{jt} + \gamma[Trend_{jt} \times 1(\tau \geq 0)_{jt}] + \theta_1[1(Winner)_{pj} \times 1(\tau \geq 0)_{jt}] + \theta_2[Trend_{jt} \times 1(Winner)_{pj} \times 1(\tau \geq 0)_{jt}] + \alpha_p + \mu_{it} + \lambda_j + \varepsilon_{pijt}$$

The analogue: to these in the current county-level context are:

$$(3)\ \ln(Y_{kjt}) = \delta 1(Winner)_{kj} + \kappa 1(\tau \geq 0)_{jt} + \theta_1[1(Winner)_{kj} \times 1(\tau \geq 0)_{jt}] + \alpha_k + \mu_t + \lambda_j + \varepsilon_{kjt}$$

$$(4)\ \ln(Y_{kjt}) = \delta 1(Winner)_{kj} + \psi Trend_{jt} + \Omega[Trend_{jt} \times 1(Winner)_{kj}] + \kappa 1(\tau \geq 0)_{jt} + \gamma[Trend_{jt} \times 1(\tau \geq 0)_{jt}] + \theta_1[1(Winner)_{5j} \times 1(\tau \geq 0)_{jt}] + \theta_2[Trend_{jt} \times 1(Winner)_{5j} \times 1(\tau \geq 0)_{jt}] + \alpha_k + \mu_t + \lambda_j + \varepsilon_{kjt},$$

where the plant fixed effects have been replaced by county fixed effects and the outcome is total employment. The inclusion of counties that appear multiple times with either the same or different winner status is required for the county, case, and the winner status dummy variables not to be perfectly collinear. For cases in which the winners and losers only appear once in the sample, the case fixed effects cannot be separated from the county fixed effects. Similar issues arise with the other parameters. This does not necessarily mean that the equations cannot be estimated, but it does just the interpretation of the results in light of the variation that is identifying the parameters.

Table A3 below contains the results from estimating Equation (3) with the full sample, a sample restricted to only those counties which don't appear as both a winner and loser for different cases, and a sample restricted to only those counties which appear only once as a winner or a loser. There are two sets of estimates for each sample that correspond to two different methods for estimating equation (3) in practice. The first (Columns 1, 3, and 5) is the method employed in GHM and this paper, which is to estimate

(5) $\ln\left(Y_{kjt}\right) = \vartheta_1\left[1(Winner)_{kj} \times 1(\tau < 0)_{jt}\right] + \vartheta_2\left[1(Winner)_{kj} \times 1(\tau \geq 0)_{jt}\right] + \pi_1\left[1(Loser)_{kj} \times 1(\tau < 0)_{jt}\right] + \pi_2\left[1(Loser)_{kj} \times 1(\tau \geq 0)_{jt}\right] + \alpha_k + \mu_t + \lambda_j + \varepsilon_{kjt}$, and calculate θ_1 as

(6) $\theta_1 = [\vartheta_2 - \vartheta_1] - [\pi_2 - \pi_1]$

The second method is to estimate Equation (3) as written. The two should be equivalent; however, the different number of indicator parameters means that the solution to the "dummy variable trap" is different for the two implementation methods and the identifying variation differs enough to result in different estimates. The underlying cause can be seen in the difference in the number of omitted case fixed effects across specifications.

Comparing the results in Panels A and B, the same number of cases are represented in both estimates but the number of omitted case fixed effects is much higher in Panel B once the counties that no longer appear as both a winner and loser are included in the sample. Because the case fixed effects can only be separately identified from the county fixed effects and DD indicators for counties that either appear more than once in the data as either a winner, loser, or both, removing the counties that appear as both a winner and loser changes the number of within case comparisons and within county variation identifying the estimates. The point is made more obvious by comparing Panels A and B to Panel C in which the sample is restricted only counties that appear once as a winner or loser. All case fixed effects are dropped in Panel C as well as the $\left[1(Winner)_{kj} \times 1(\tau \geq 0)_{jt}\right]$ and $\left[1(Loser)_{kj} \times 1(\tau \geq 0)_{jt}\right]$ parameter estimates. The DD estimates are identified solely from the pooled time variation.

Table A3: Change in winner county employment

	A. Full Sample		B. Excluding Counties with Both Winner and Loser Status		C. Restricted to Counties Appearing Once as a Winner or Loser	
	(1)	(2)	(3)	(4)	(5)	(6)
θ_1	0.0455*	0.0370	0.0492*	0.0420	0.0397	0.0327
	(0.0266)	(0.0229)	(0.0283)	(0.0255)	(0.0306)	(0.0266)
δ		0.0179		0.0470	omitted	omitted
		(0.0394)		(0.0339)		
ϑ_1	0.0202		0.0831***		-0.0199	
	(0.0589)		(0.0289)		(0.0217)	
ϑ_2	-0.00102		0.111***		omitted	
	(0.0613)		(0.0193)			
π_1	0.0202		0.0389***		0.0198	
	(0.0589)		(0.0148)		(0.0172)	
π_2	-0.00102		0.0175		omitted	
	(0.0613)		(0.0199)			
Omitted μ_t	3	3	3	3	3	3
Omitted λ_j	36	35	44	43	56	56
Total Cases	64	64	64	64	56	56

To demonstrate the issues associated with inclusion of the relocations (i.e., inclusion of cases where the loser was the county where the firm was closing a plant), I repeat the exercise above but drop all relocation cases.

Table A4: Sensitivity to inclusion of relocation cases

	A. Full Sample without Relocations		B. Excluding Counties with Both Winner and Loser Status		C. Restricted to Counties Appearing Once as a Winner or Loser	
	(1)	(2)	(3)	(4)	(5)	(6)
θ_1	0.0251	0.0229	0.0265	0.0250	0.0140	0.0126
	(0.0326)	(0.0288)	(0.0341)	(0.0310)	(0.0363)	(0.0316)
δ		0.0737***		-0.0746*	omitted	omitted
		(0.0197)		(0.0392)		
ϑ_1	0.0511**		0.0416**		-0.00990	
	(0.0203)		(0.0172)		(0.0264)	
ϑ_2	0.0704***		0.0371		omitted	
	(0.0254)		(0.0241)			
π_1	-0.0220		-0.0334		0.00411	
	(0.0301)		(0.0331)		(0.0211)	
π_2	-0.0278		-0.0114		omitted	
	(0.0309)		(0.0223)			

B. *Equation (1) event study tests of identifying assumptions*

Figure B1: Spillover event study GHM winners and losers

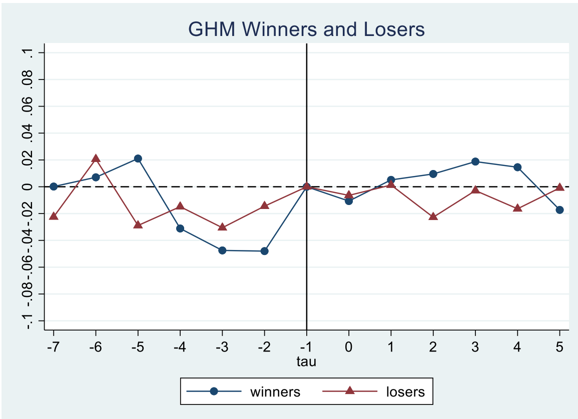


Figure B2: Spillover event study Case set 1 and propensity score losers

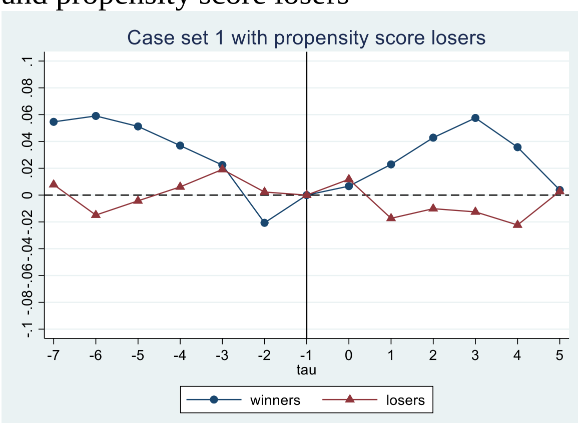


Figure B3: Spillover event study Case set 2 and propensity score losers

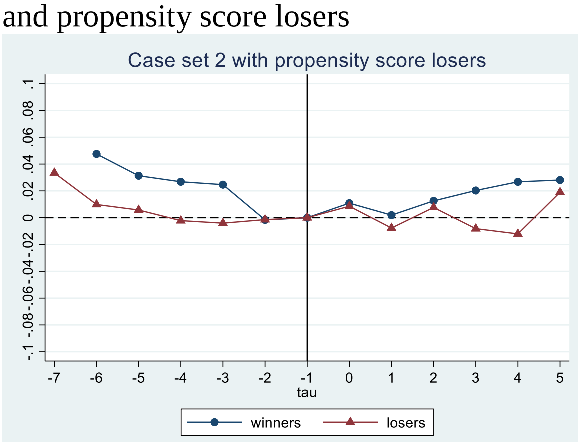


Figure B4: Spillover event study Case set 3 and propensity score losers

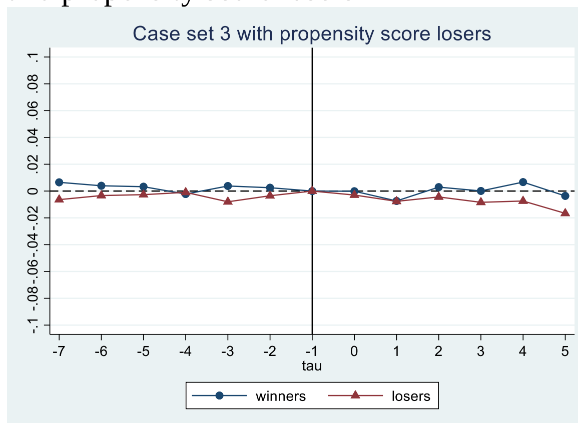


Figure B5: Spillover event study Case set 4 and propensity score losers

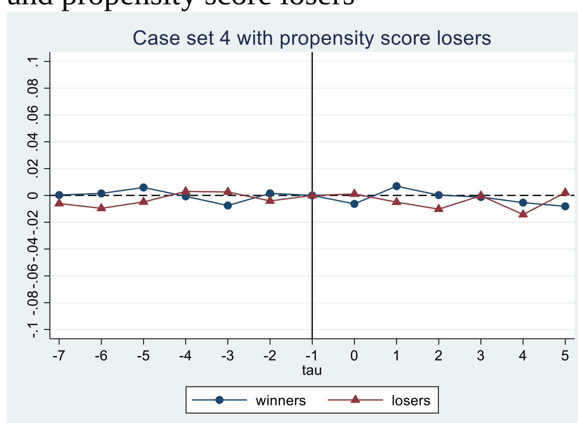


Table B1: Case Set 1 Incumbent Plant Productivity Relative to year before an MDP Opening

	Panel A			Panel B		
	In Winner Counties	In GHM Loser Counties	Difference Col. 1 – Col. 2	In Winner Counties	In Pscore Loser Counties	Difference Col. 1 – Col. 2
	(1)	(2)	(3)	(1)	(2)	(3)
$\tau = -7$	1.652e-04	-0.02247	0.02263	0.05465**	0.007813	0.04684*
	(0.03650)	(0.03387)	(0.02336)	(0.02477)	(0.02846)	(0.02746)
$\tau = -6$	-0.007045	-0.02064	0.01360	0.05903***	-0.01489	0.07392***
	(0.03147)	(0.03107)	(0.02682)	(0.02120)	(0.02321)	(0.02633)
$\tau = -5$	-0.02105	-0.02890	0.007854	0.05124**	-0.004284	0.05553***
	(0.02982)	(0.02720)	(0.02823)	(0.02123)	(0.01935)	(0.02090)
$\tau = -4$	-0.03112	-0.01485	-0.01626	0.03693*	0.006134	0.03079
	(0.02536)	(0.02492)	(0.02507)	(0.01885)	(0.01671)	(0.01914)
$\tau = -3$	-0.04749*	-0.03055	-0.01694	0.02242	0.01915	0.003274
	(0.02771)	(0.01920)	(0.02629)	(0.01706)	(0.01307)	(0.01971)
$\tau = -2$	-0.04800	-0.01447	-0.03353	-0.02067	0.002289	-0.02296
	(0.03242)	(0.01141)	(0.03196)	(0.02664)	(0.01565)	(0.02941)
$\tau = -1$	-	-	-	-	-	-
$\tau = 0$	-0.01066	-0.006441	-0.004218	0.006673	0.01176	-0.005082
	(0.01833)	(0.01983)		(0.02016)	(0.01212)	(0.02203)
$\tau = 1$	0.005134	0.001288	0.003845	0.02292	-0.01731	0.04023
	(0.02184)	(0.01645)	(0.02352)	(0.02425)	(0.01765)	(0.02677)
$\tau = 2$	0.009550	-0.02272	0.03227	0.04285**	-0.01012	0.05297**
	(0.03011)	(0.02356)	(0.03719)	(0.02182)	(0.01970)	(0.02640)
$\tau = 3$	0.01878	-0.002844	0.02162	0.05755**	-0.01251	0.07006**
	(0.03344)	(0.02294)	(0.02879)	(0.02326)	(0.02598)	(0.03109)
$\tau = 4$	0.01457	-0.01642	0.03099	0.03573	-0.02228	0.05801
	(0.03360)	(0.02710)	(0.03010)	(0.03015)	(0.02990)	(0.03615)
$\tau = 5$	-0.01726	-8.531e-04	-0.01640	0.003816	0.002810	0.001005
	(0.04067)	(0.03674)	(0.03751)	(0.03661)	(0.03513)	(0.03736)
Obs.		27,000			17,500	
R^2		0.985			0.987	

Notes: The table presents the results of estimating the production function with an interaction terms for every winner status-relative year combination. The coefficients in the winner (loser) columns are then calculated as the difference between the winner (loser) – relative year coefficient and the winner (loser) coefficient in year $\tau = -1$. Panel A uses the sample of incumbent plants in GHM winner and loser counties to the extent that this sample was replicable using the restricted-access replication code provided by the Census and the information in Greenstone and Moretti (2003). Panel B uses the GHM sample of winning counties and incumbent plants in the nearest two propensity score counties within 100-250 miles of the winning county for each case.

Table B2: Case Sets 2-4 Incumbent Plant Productivity Relative to year before an MDP Opening

	Panel A: Case set 2			Panel B: Case set 3			Panel C: Case set 4		
	In Winner Counties (1)	In GHM Loser Counties (2)	Difference Col. 1 – Col. 2 (3)	In Winner Counties (1)	In GHM Loser Counties (2)	Difference Col. 1 – Col. 2 (3)	In Winner Counties (1)	In GHM Loser Counties (2)	Difference Col. 1 – Col. 2 (3)
$\tau = -7$	0.04755**	0.03344	3.129e-04	0.006518	-0.006391	0.01291**	3.149e-04	-0.006020	0.006335
	(0.02073)	(0.02276)	(0.02186)	(0.004419)	(0.004080)	(0.005647)	(0.007383)	(0.008175)	(0.009781)
$\tau = -6$	0.03129**	0.009850	0.03770*	0.003962	-0.003359	0.007320	0.001490	-0.009602	0.01109
	(0.01545)	(0.01726)	(0.02168)	(0.004219)	(0.003929)	(0.005473)	(0.007405)	(0.008159)	(0.01011)
$\tau = -5$	0.02680**	0.005736	0.02555	0.003259	-0.002685	0.005944	0.005938	-0.004839	0.01078
	(0.01182)	(0.01426)	(0.01754)	(0.004068)	(0.003823)	(0.005409)	(0.007903)	(0.009820)	(0.01187)
$\tau = -4$	0.02466**	-0.002197	0.02900	-0.002228	-9.451e-04	-0.001283	-6.908e-04	0.002977	-0.003667
	(0.01137)	(0.01535)	(0.01831)	(0.004063)	(0.003755)	(0.005514)	(0.006183)	(0.008774)	(0.01012)
$\tau = -3$	-0.001621	-0.004063	0.02872**	0.003782	-0.007999**	0.01178**	-0.007537	0.002606	-0.01014
	(0.01415)	(0.01191)	(0.01351)	(0.003953)	(0.003712)	(0.005399)	(0.006821)	(0.009040)	(0.01116)
$\tau = -2$	0.04755**	-0.001407	-2.134e-04	0.002479	-0.003497	0.005976	0.001558	-0.004078	0.005636
	(0.02073)	(0.01299)	(0.01868)	(0.004081)	(0.003775)	(0.005571)	(0.005278)	(0.006141)	(0.007682)
$\tau = -1$	-	-	-	-	-	-	-	-	-
$\tau = 0$	0.01085	0.008565	0.002286	-1.238e-04	-0.002961	0.002837	-0.006289	0.001089	-0.007379
	(0.01054)	(0.01097)	(0.01418)	(0.004112)	(0.003840)	(0.005640)	(0.005985)	(0.006363)	(0.008308)
$\tau = 1$	0.001929	-0.007688	0.009617	-0.007385*	-0.007664**	2.785e-04	0.006882	-0.004988	0.01187
	(0.01156)	(0.01731)	(0.01786)	(0.004058)	(0.003838)	(0.005584)	(0.005544)	(0.007248)	(0.009516)
$\tau = 2$	0.01253	0.007637	0.004891	0.002934	-0.004395	0.007328	2.249e-04	-0.01022	0.01045
	(0.01438)	(0.01501)	(0.01733)	(0.004293)	(0.004047)	(0.005922)	(0.008545)	(0.008265)	(0.01257)
$\tau = 3$	0.02030	-0.008174	0.02848	9.373e-05	-0.008395*	0.008489	-0.001211	-2.270e-04	-9.837e-04
	(0.01960)	(0.01617)	(0.02325)	(0.004546)	(0.004344)	(0.006202)	(0.01034)	(0.009073)	(0.01451)
$\tau = 4$	0.02676	-0.01207	0.03883	0.006680	-0.007428	0.01411**	-0.005362	-0.01420	0.008835
	(0.01994)	(0.01748)	(0.02604)	(0.004832)	(0.004564)	(0.006461)	(0.008397)	(0.009138)	(0.01102)
$\tau = 5$	0.02810	0.01899	0.009116	-0.003630	-0.01662***	0.01299*	-0.008107	0.002079	-0.01019
	(0.02253)	(0.02108)	(0.02243)	(0.005155)	(0.004803)	(0.006841)	(0.008785)	(0.01258)	(0.01365)

Obs.	30,500	93,500	123,000
R^2	0.983	0.981	0.978

NOTES The table presents the results of estimating the production function with an interaction terms for every winner status-relative year combination. The coefficients in the winner (loser) columns are then calculated as the difference between the winner (loser) – relative year coefficient and the winner (loser) coefficient in year $\tau = -1$. Each panel represents results for estimation using the respective case set samples of continuously-appearing incumbent plants in the winning counties and their nearest two propensity score neighbors. Standard errors clustered at the county level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

C. *Equation (7) event study tests of identifying assumptions using nearest MDP-industry location quotient neighbor counterfactuals*

Table C1: Nearest Location Quotient Incumbent Plant Summary Statistics

	Case Set 1		Case set 2		Case set 3		Case set 4	
	Winners	LQ Losers	Winners	LQ Losers	Winners	LQ Losers	Winners	LQ Losers
Plants								
(log) Output	10.6	10.6	10.48	10.41	10.54	10.55	10.51	10.52
	1.133	0.9826	1.09	1.026	1.096	1.079	1.088	1.07
(log) Labor	6.417	6.455	6.411	6.412	6.339	6.342	6.261	6.278
	1.011	1.049	0.9633	0.891	0.9868	0.9858	1.012	0.9919
Counties								
Incumbent Plants	10.76	11.79	14.76	5.417	14.42	6.662	19.23	9.777
	12.31	13.62	40.16	6.857	32.05	10.22	43.38	32.46
Counties	40	80	70	100	300	700	300	500
Total Counties	100		200		650		700	

Notes: The table presents sample statistics for the samples of continuously appearing incumbent plants in the treated ("winner") and control ("loser") counties for each set of MDP cases. Case set 1 is the sample MDPs in Greenstone, Hornbeck, and Moretti (2010) (GHM). Case set 2 includes the GHM MDPs that appear in Site Selection magazine and none of the GHM MDPs that do not appear in the magazine as well as a subset of any other MDP appearing in Site Selection magazine during the GHM sample period and a subset of incentivized plant locations reported in the Good Jobs First Subsidy Tracker Database from 1988-1993. Case set 3 includes the case set 2 MDPs plus a subset of large, incentivized plants appearing in the Good Jobs First Subsidy Tracker from 1994-1997. Case set 4 is a random sample of 500 new establishment births from the set of new establishment births in the micro data that had the employment above the 95th percentile of employment in new births from 1982-1997. Counties represents the number of unique counties from which the samples are drawn. The number of counties is weighted by the inverse of their number per case for cases to receive equal weight. Plants are weighted by the product of the inverse of their number per county and the inverse of the number of counties per case. The number of counties and all statistics are rounded according to the Census rounding rules for disclosure.

Figure C1: Spillover event study Case set 1 and location quotient losers

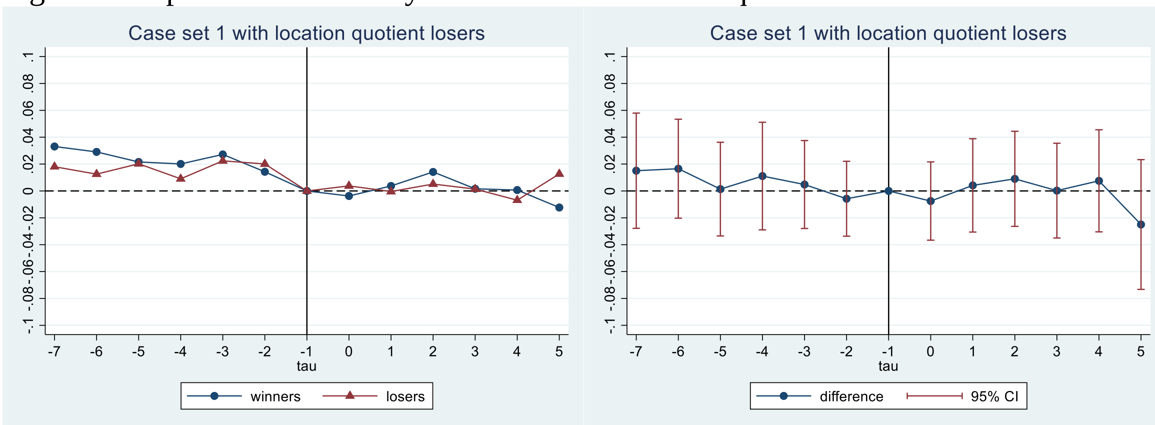


Figure C2: Spillover event study Case set 2 and location quotient losers

Figure C3: Spillover event study Case set 3 and location quotient losers

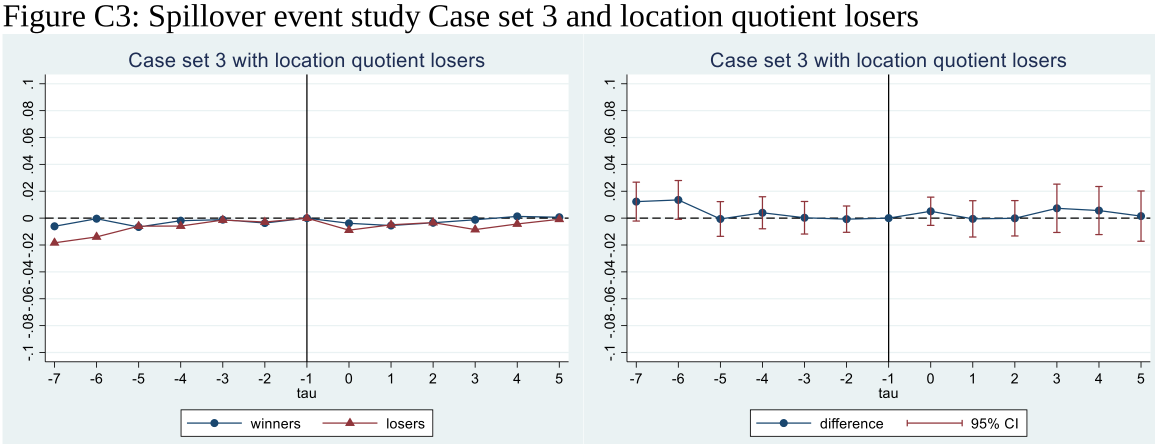
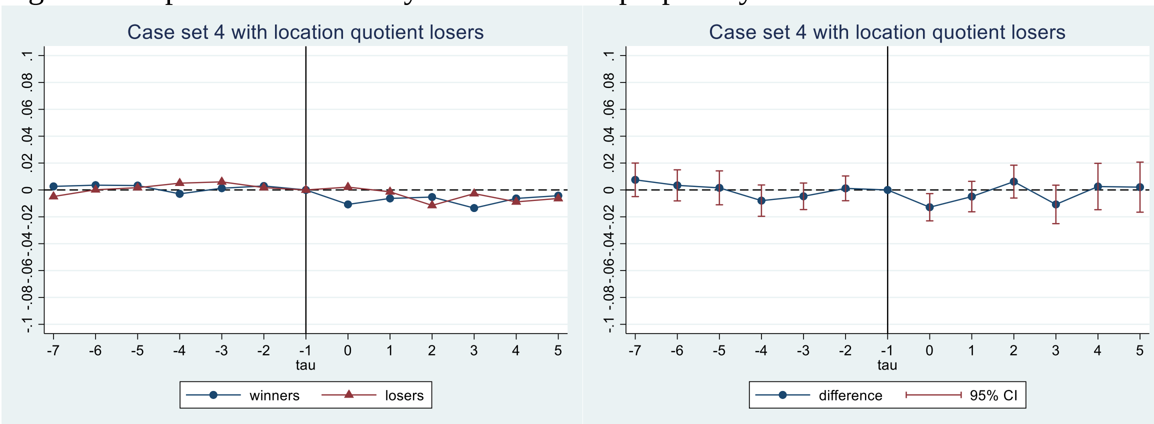


Figure C4: Spillover event study Case set 4 and propensity score losers



D. Multiple Equilibria Analysis

D.1 Methodology

We test the multiple equilibria hypothesis using the methodologies developed in Davis and Weinstein (2002; 2008) and Bosker et al. (2007). The methodology compares county-manufacturing and county-manufacturing-industry shares of national manufacturing and manufacturing-industry from before the MDP location with those 20 years after. We define county-manufacturing (log) share at time τ in county c receiving MDP shock j as its location quotient $s_{c(j)\tau} \equiv \ln\left(S_{c(j)\tau}\right) = \ln\left(\frac{manufacturing\ output_{c(j)\tau}}{manufacturing\ output_{US\tau}}\right)$.[4] Assuming a unique, stable equilibrium, county manufacturing shares at time τ can be modeled as $s_{c(j)\tau} = \Pi_{c(j)} + \varepsilon_{c(j)\tau}$, where $\Pi_{c(j)}$ is the initial equilibrium size in county c and $\varepsilon_{c(j)\tau}$ is a location-specific shock to manufacturing share. Persistence of shocks takes the form $\varepsilon_{c(j),\tau+1} = \rho\varepsilon_{c(j),\tau} + \nu_{c(j),\tau+1}$, where $\rho \in [0,1)$ is the persistence parameter.

Let $\nu_{c(j),5}$ be the MDP shock to county output during the first five years after opening and $\nu_{c(j),20}$ be the typical idiosyncratic location-specific shock to manufacturing share around the new post-MDP equilibrium $s_{c(j),20}$. As shown in Davis and Weinstein (2002), our equation for the effect of the MDP shock to winning county c(j)'s share of manufacturing output becomes:

[4] We measure the location's manufacturing output share from the data and use all plants reporting.

$$(7) \qquad s_{c(j),20} - s_{c(j),5} = (\rho - 1)\nu_{c(j),5} + \left[\nu_{c(j),20} + \rho(1-\rho)\varepsilon_{c(j),-5}\right],$$ [5]

where the term in brackets is the error term and uncorrelated with MDP shock. We use Census micro data on the large firm locations and therefore know the size of the MDP shock. We define the MDP shock as $MDPshock = \max\left(MDP\ output_{c(j)\tau}\right), \tau \in (0,5)$, or the maximum amount of output reported by an MDP owned firm in the winning county from the announcement date through the five full years of operation by which time we expect the MDP to be at its stable size. We then define the shock to winning county output shares as $\nu_{c(j),5} \equiv MDPshock/output_{c(j),-1}$, which we can relate to the growth rate in winning county manufacturing shares during this period by noting $s_{c(j),5} - s_{c(j),-1} \approx \left(\frac{\frac{mfg\ output_{c(j),-1} + MDP\ output_{c(j),5}}{mfg\ output_{US,-1}} - \frac{mfg\ output_{c(j),-1}}{mfg\ output_{US,-1}}}{\frac{mfg\ output_{c(j),-1}}{mfg\ output_{US,-1}}}\right) = \frac{MDP\ output_{c(j),-1}}{mfg\ output_{c(j),-1}}$.

The unique equilibrium test rests on the estimation of ρ. If $\rho = 1$, then the shock is permanent and shares follow a random walk. If $\rho = 0$, then the shock dissipates fully. $0 \leq \rho < 1$ suggests a mean-reverting process and we can reject that manufacturing shares follow a random walk. This may or may not be consistent with multiple equilibria.

It is possible that $\rho \neq 0$ because there is some correlation between the future changes in county manufacturing shares and past changes that we do not model. Thus, we include pre-MDP opening growth in manufacturing share as a control in the estimating equation. It is also possible that the MDP shock is correlated with the error term and we cannot obtain a consistent estimate of ρ by directly estimating (7). Instead, we instrument for the MDP shock using average national establishment output for firms in the MDP's 4-digit SIC industry and average national new entrant output in the MDP's 3-digit industry in time $\tau = -1$ expressed as a share of initial winning county manufacturing output. We use instruments from $\tau = -1$ to avoid any concerns

[5] As suggested in Bosker et al. (2007), we use growth rates constructed as $\frac{x_t - x_{t-i}}{x_{t-i}}$, rather than $\ln(x_t) - \ln(x_{t-i})$ because the latter is only a valid measure of growth for small changes.

that MDP's output decision or announcement influences the output decisions of other firms.[6] Our estimating equation therefore becomes:

$$(8) \qquad s_{c(j),20} - s_{c(j),5} = \alpha \hat{v}_{c(j),5} + \beta_0 + \zeta PreMDP_{c(j)} + error_{c(j)}{}^{7},$$

where $\alpha = \rho - 1$ and $PreMDP_{c(j)}$is the pre-MDP opening growth in county manufacturing share. Under the null of hypothesis of a unique equilibrium, the coefficient on the instrumented MDP shock is minus one.

The coefficient on the MDP shock from equation (8) gives evidence about whether the data support rejection of the null of a unique equilibrium, it does not provide a direct test whether the data support a unique equilibrium over multiple equilibria. Under the assumption of multiple equilibria, a large enough positive (negative) shock to manufacturing output can push the county's share past some threshold beyond which the county has a higher (lower) equilibrium share. In the case of three equilibria, for example, a county's share of manufacturing output at the new post-MDP equilibrium may be written:

$$(9) \qquad s_{c(j),20} = \begin{cases} \Pi_{c(j)} + \Delta_1 + \varepsilon^1_{c(j),20} & if\ v_{c(j),5} < b_1 \\ \Pi_{c(j)} + \varepsilon^2_{c(j)20} & if\ b_1 < v_{c(j),5} < b_2 \\ \Pi_{c(j)} + \Delta_3 + \varepsilon^3_{c(j)20} & if\ v_{c(j),5} > b_2 \end{cases}$$

where $\Delta_1$and Δ_3 are the respective differences in log-shares from the initial equilibrium and the new equilibrium, $b_1$and $b_2$are the respective thresholds, and the equilibrium specific error terms are as follows:

$$(10) \qquad \begin{aligned} \varepsilon^1_{c(j),20} &= \rho(\varepsilon_{c(j),5} - \Delta_1) + v_{c(j)20} \quad if\ v_{c(j),5} < b_1 \\ \varepsilon^2_{c(j),20} &= \rho\varepsilon_{c(j),5} + v_{c(j),20} \quad if\ b_1 < v_{c(j),5} < b_2 \\ \varepsilon^3_{c(j),20} &= \rho(\varepsilon_{c(j),5} - \Delta_3) + v_{c(j),20} \quad if\ v_{c(j),5} > b_2 \end{aligned}$$

[6] In the spirit of Bosker et al. (2007), we also consider a geographical extension and used the distance-weighted change in surrounding county manufacturing shares as an instrument; however, conditional on our other instruments this had little power in the first stage and we therefore report estimates using the MDP industry output instruments.

[7] We include a constant to allow for the possibility that the error is not mean zero. Shares do not add to one.

As shown in Davis and Weinstein (2008), taken together, these give:

$$(11) \quad s_{c(j),20} - s_{c(j),5} =$$

$$\begin{cases} \Delta_1(1-\rho) + (\rho-1)\nu_{c(j),5} + \left[v_{c(j),20} + (1-\rho)\varepsilon_{c(j),-5}\right] & if\ \nu_{c(j),5} < b_1 \\ (\rho-1)\nu_{c(j),5} + \left[v_{c(j),20} + (1-\rho)\varepsilon_{c(j),-5}\right] & if\ b_1 < \nu_{c(j),5} < b_2 \\ \Delta_3(1-\rho) + (\rho-1)\nu_{c(j),5} + \left[v_{c(j),20} + (1-\rho)\varepsilon_{c(j),-5}\right] & if\ \nu_{c(j),5} > b_2 \end{cases}$$

The equations in (11) have different constants, but are otherwise the same. The term in brackets is the error term and uncorrelated with the MDP shock. If shocks are not very persistent, then $\rho = 0$. The expression in (11) can be rewritten as:

$$(12) \quad s_{c(j),20} - s_{c(j),5} = (1-\rho)\Delta_1 I_1\left(b_1, \nu_{c(j),5}\right) + (1-\rho)\Delta_3 I_3\left(b_2, \nu_{c(j),5}\right) + (\rho-1)\left(s_{c(j),5} - s_{c(j),-1}\right) + \left[v_{c(j),20} + (1-\rho)\varepsilon_{c(j),-5}\right],$$

where $I_1\left(b_1, \nu_{c(j),5}\right)$ is an indicator variable equal to one if $\nu_{c(j),5} < b_1$ and $I_3\left(b_2, \nu_{c(j),5}\right)$ is an indicator variable equal to one if $\nu_{c(j),5} > b_2$. As in the previous literature, we assume that the period is long enough for the shock to have dissipated and thus $\rho = 0$ and estimate:

$$(13) \quad s_{c(j),20} - s_{c(j),-1} = (1-\rho)\Delta_1 I_1\left(b_1, \widehat{\nu_{c(j),5}}\right) + (1-\rho)\Delta_3 I_3\left(b_2, \widehat{\nu_{c(j),5}}\right) + \zeta PreMDP_{c(j)} + v_{c(j),20},$$

where we use instrumented MDP shocks and include the PreMDP growth rate in county manufacturing shares to address any concerns over potential correlation between the error term and the shock.

Equation (13) assumes we know the number of equilibria and the value of the thresholds. In practice, we do not and use the maximum likelihood grid search method and selection criteria described in Davis and Weinstein (2008) and Bosker et al (2007). We consider one, two, three, and four equilibria specifications and use the value of the likelihood functions to determine which bests describes the data.

The multiple equilibria estimating equations require a comparison of winning county shares of national manufacturing output over time. We construct these shares from the restricted-access

Census of Manufacturers (CM). We remove any plants owned by the MDP firms from the micro data and then aggregate the microdata by county-year and county-industry-year. We require years that are not CM years and therefore linearly interpolate between CM years for each county and the nation as a whole. We do not construct shares from ASM data for these years because the ASM sampling scheme could introduce errant variation in shares.

D.2 Results

Tables D1-D4 present the results of our test of the multiple equilibria hypothesis in county manufacturing shares using winning counties from case sets 1, 2, 3, and 4, respectively. Tables D5-D6 report the analogous results for multiple equilibria in county-MDP industry shares. Column 1 in each table present the results of instrumental variable estimation of equation (7), which tests the null of a unique equilibrium by testing with the coefficient on the MDP shock is minus unity. Columns 2-5 present the results of the one, two, three, and four equilibria specifications of equation (13) with the threshold values that maximized the Schwarz criterion in the maximum likelihood grid search. We choose the number of equilibria best supported by the data by choosing the specification that maximizes the Schwarz criterion and requiring, if it is a multiple equilibria specification, that the intercepts and thresholds take sensible values. In particular, we require that a larger positive shock be associated with larger, new equilibrium share and that the thresholds lie between equilibrium shares. Following Davis and Weinstein (2008), we therefore impose the following intercept ordering criterion: $\delta_1 + \delta_2 < b_1 < \delta_2 < b_2 < \delta_2 + \delta_3 < b_3 < \delta_2 + \delta_4$.

Table D1 presents the manufacturing share results for case set 1 winning counties, the counties receiving the GHM MDP shocks. Recall that this was the only case set for which we estimated positive spillovers and therefore the most likely candidate for shocks sufficient to push the winning counties into a new equilibrium. We can reject that the coefficient on the MDP shock is minus unity in the IV specification reported in Table 10 Column 1 – meaning that we can reject the null of a unique equilibrium. The specifications in Columns 2-5 put a unique equilibrium within the same framework as multiple equilibria. Comparing the Schwarz criterion across specifications, the data most strongly support a unique equilibrium despite the IV rejection of the unique equilibrium null. Among the multiple equilibria specifications, the two equilibria specification is preferred and also passes the intercept ordering criterion.

Table D2 presents the estimates for counties winning case set 2 MDPs, which represented the largest share of winning county output, on average, in a samples of MDP cases. Here, we can reject the null hypothesis that the coefficient on the MDP shock is minus unity in the IV specification (Column 1). Again, this suggests that we can reject the null hypothesis of one unique equilibrium. However, the threshold regressions suggest that the unique equilibrium specification better describes the data than any of the multiple equilibria specifications. Among the multiple equilibria specifications, the three equilibria specification is slightly preferred to the two equilibria specification.

Tables D3 and D4 present the results for case sets 3 and 4, respectively. In both cases, the IV specification indicates that we can reject the null of a unique equilibria, but the Schwarz criterion prefers the unique equilibria specification. The rejection of the coefficient on the MDP shock as equal to minus unity in the IV specifications suggests that $\rho \neq 0$, which is the assumption imposed by our multiple equilibria tests. However, the estimated coefficients are positive suggesting that $\rho > 0$. As discussed in Davis and Weinstein (2008), $\rho > 0$ would bias our multiple equilibria tests in favor of finding multiple equilibria. The fact that the data are still best described by the unique equilibrium specification provides even stronger evidence in support of a unique equilibrium

Table D1: Case set 1 manufacturing share multiple equilibria test

	$s_{c(j),20} - s_{c(j),5}$	$s_{c(j),20} - s_{c(j),-1}$			
	IV Estimate (1)	1 Equilibrium (2)	2 Equilibria (3)	3 Equilibria (4)	4 Equilibria (5)
PreMDP Growth Rate	0.06642	1.325***	1.572***	1.29*	2.824***
	(0.3079)	(0.4708)	(0.5147)	(0.5035)	(0.7544)
MDP Shock	-0.05057				
	(0.05477)				
δ_1			-1.771***	1.064**	-4.010**
			(0.5575)	(0.5036)	(1.628)
δ_3				1.952***	-1.323***
				(0.5328)	(0.4536)
δ_4					1.987***
					(0.4928)
Constant	0.2208*	0.4331**	2.004***	0.07808	1.224***
	(0.1215)	(0.1836)	(0.5218)	(0.2150)	(0.4043)
Thresholds					
b_1			0.3323	-0.7615	-4.253
b_2				0.4855	-0.3208
b_3					0.4855
Intercept ordering criterion	N/A	N/A	Pass	Fail	Fail
Schwarz Criterion	N/A	-46.57	-52.18	-53.25	-53.56
Counties	30	30	30	30	30
R-squared		0.215	0.397	0.483	0.577

NOTES: The table presents the results of our tests for manufacturing share multiple equilibria associated with the case set 1 MDPs. Column 1 is the IV estimation of equation (12), which tests the null of a unique equilibrium by testing with the coefficient on the MDP shock is minus unity. Columns 2-5 present the results of the one, two, three, and four equilibria specifications of equation (17) with the threshold values that maximized the Schwarz criterion. The intercept ordering criterion requires: $\delta_1 + \delta_2 < b_1 < \delta_2 < b_2 < \delta_2 + \delta_3 < b_3 < \delta_2 + \delta_4$, where the constant is δ_2 above. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D2: Case set 2 manufacturing share multiple equilibria test

	$s_{c(j),20} - s_{c(j),5}$	$s_{c(j),20} - s_{c(j),-1}$			
	IV Estimate (1)	1 Equilibrium (2)	2 Equilibria (3)	3 Equilibria (4)	4 Equilibria (5)
PreMDP Growth Rate	-3.193*	0.6045	1.515	1.462	2.185*
	(1.723)	(0.8233)	(1.200)	(1.137)	(1.291)
MDP Shock	0.09841*				
	(0.05061)				
δ_1			-5.165***	-0.8436	2.360
			(1.267)	(1.26)	(2.518)
δ_3				6.488***	2.830
				(1.742)	(2.496)
δ_4					6.931***
					(1.428)
Constant	0.1606	0.9163***	5.351***	0.8958	-2.413
	(0.1764)	(0.3199)	(1.169)	(1.174)	(2.460)
Thresholds					
b_1			1.076	0.6336	0.04455
b_2				1.739	0.0716
b_3					1.739
Intercept ordering criterion	N/A	N/A	Pass	Pass	Fail
Schwarz Criterion	N/A	-182.8	-214.4	-214	-217.8
Counties	70	70	70	70	70
R-squared		0.007	0.199	0.295	0.304

NOTES: The table presents the results of our tests for manufacturing share multiple equilibria for case set 2 MDPs. Column 1 is the IV estimation of equation (12), which tests the null of a unique equilibrium by testing with the coefficient on the MDP shock is minus unity. Columns 2-5 present the results of the one, two, three, and four equilibria specifications of equation (17) with the threshold values that maximized the Schwarz criterion. The intercept ordering criterion requires: $\delta_1 + \delta_2 < b_1 < \delta_2 < b_2 < \delta_2 + \delta_3 < b_3 < \delta_2 + \delta_4$, where the constant is δ_2 above. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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Table D3: Case set 3 manufacturing share multiple equilibria test

	$s_{c(j),20} - s_{c(j),5}$	$s_{c(j),20} - s_{c(j),-1}$			
	IV Estimate (1)	1 Equilibrium (2)	2 Equilibria (3)	3 Equilibria (4)	4 Equilibria (5)
PreMDP Growth Rate	-0.08337	-0.05676	-0.2042	-0.2420	-0.2443
	(0.06114)	(0.1150)	(0.1638)	(0.1570)	(0.1570)
MDP Shock	0.06165**				
	(0.02390)				
δ_1			-3.053***	-0.7947**	-0.2749
			(0.3346)	(0.3853)	(0.2548)
δ_3				3.553***	0.6676*
				(0.5121)	(0.4021)
δ_4					3.549***
					(0.5088)
Constant	-0.02123	0.4791***	3.212***	0.9009**	0.2414
	(0.04595)	(0.08505)	(0.3107)	(0.3643)	(0.1815)
Threshholds					
b_1			0.3316	0.23	0.0244
b_2				0.5663	0.23
b_3					0.5663
Intercept ordering criterion	N/A	N/A	Pass	Fail	Fail
Schwarz Criterion	N/A	-851	-1006	-993.4	-998.8
Counties	450	450	450	450	450
R-squared		0.001	0.164	0.234	0.237

NOTES: The table presents the results of our tests for manufacturing share multiple equilibria for case set 3 MDPs. Column 1 is the IV estimation of equation (12), which tests the null of a unique equilibrium by testing with the coefficient on the MDP shock is minus unity. Columns 2-5 present the results of the one, two, three, and four equilibria specifications of equation (17) with the threshold values that maximized the Schwarz criterion. The intercept ordering criterion requires: $\delta_1 + \delta_2 < b_1 < \delta_2 < b_2 < \delta_2 + \delta_3 < b_3 < \delta_2 + \delta_4$, where the constant is δ_2 above. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table D4: Case set 4 manufacturing share multiple equilibria test

	$s_{c(j),20} - s_{c(j),5}$	$s_{c(j),20} - s_{c(j),-1}$			
	IV Estimate (1)	1 Equilibrium (2)	2 Equilibria (3)	3 Equilibria (4)	4 Equilibria (5)
PreMDP Growth Rate	0.08275	0.3628***	0.3015***	0.3025***	0.3019***
	(0.2011)	(0.02336)	(0.02776)	(0.02774)	(0.02766)
MDP Shock	0.6852				
	(0.4926)				
δ_1			-0.3799***	-0.8039***	-0.4935**
			(0.09322)	(0.2157)	(0.2211)
δ_3				-0.4937**	-0.2173*
				(0.2217)	(0.1202)
δ_4					0.7127***
					(0.2210)
Constant	-0.06347	-3.856e-04	0.1892***	0.6729***	-0.03981
	(0.04527)	(0.03918)	(0.06289)	(0.2074)	(0.07788)
Thresholds					
b_1			0.0003642	0.0007467	0.00009817
b_2				0.001002	0.0007538
b_3					0.001002
Intercept ordering criterion	N/A	N/A	Pass	Fail	Fail
Schwarz Criterion	N/A	-484.9	-559	-563.1	-567.5
Counties	400	400	400	400	400
R-squared		0.374	0.262	0.269	0.275

NOTES: The table presents the results of our tests for manufacturing share multiple equilibria for case set 4 MDPs. Column 1 is the IV estimation of equation (12), which tests the null of a unique equilibrium by testing with the coefficient on the MDP shock is minus unity. Columns 2-5 present the results of the one, two, three, and four equilibria specifications of equation (17) with the threshold values that maximized the Schwarz criterion. The intercept ordering criterion requires: $\delta_1 + \delta_2 < b_1 < \delta_2 < b_2 < \delta_2 + \delta_3 < b_3 < \delta_2 + \delta_4$, where the constant is δ_2 above. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Tables D5-D8 report analogous estimates for county MDP-industry shares rather than manufacturing shares. The sample of winning counties decreases for these results because it is limited to those winning counties that had pre-existing establishments in the MDPs own 3-digit SIC code industry. We reject the null of a unique equilibrium under the IV specification for all 4 case sets. Yet, the data most strongly support a unique equilibrium when analyzed within the same framework as multiple equilibria (Columns 2-5) for case sets 1-3. Interestingly, though, the data most strongly support multiple equilibria for case set 4, with the two equilibria specification maximizing the Schwarz criterion and passing the intercept ordering criterion.

Recall from our earlier discussion that case set 4 is the set of randomly drawn largest employers in the micro data – not a set of highly incentivized large plants. These large plant shocks were, on average, much smaller output and employment shocks than the sets of highly, incentivized plants. However, they were much larger payroll shocks, potentially suggesting more highly skilled employees and higher labor productivity. Thus, it is possible that it is the size of the human capital shock, rather than the size of the output shock, that matters most for pushing a location into a new equilibrium share of manufacturing-industry output. The fact that the data does not support multiple equilibria for case set 3 shocks, which also have at least some high human capital MDPs, casts some doubt on this. Instead, it may be that multiple equilibria are supported precisely because the case set 4 shocks are not highly-incentivized and thus forego some of the general equilibrium congestion costs and crowd-out effects associated with incentives.

Table D5: Case set 1 manufacturing industry share multiple equilibria test

	$s_{c(j),20} - s_{c(j),5}$	$s_{c(j),20} - s_{c(j),-1}$			
	IV Estimate (1)	1 Equilibrium (2)	2 Equilibria (3)	3 Equilibria (4)	4 Equilibria (5)
PreMDP Growth Rate	-0.003255	-3.613	0.7158	0.7424	2.208
	(0.005816)	(9.779)	(65.93)	(69.19)	(125.1)
MDP Shock	8.568e-05***				
	(1.784e-05)				
δ_1			-41,040***	-72.48	470
			(12,870)	(18,280)	(32,880)
δ_3				40,970*	522.8
				(21,420)	(36,060)
δ_4					41,000**
					(18,340)
Constant	0.2928	729.7	41,020***	49.02	-505.4
	(0.4315)	(700.8)	(11,840)	(17,490)	(34,100)
Thresholds					
b_1			247.8	72.68	17.02
b_2				254.6	64.92
b_3					254.6
Intercept ordering criterion	N/A	N/A	Pass	Fail	Fail
Schwarz Criterion	N/A	-141.7	-172.3	-175	-177.7
Counties	20	20	20	20	20
R-squared		0.010	0.464	0.464	0.464

NOTES: The table presents the results of our tests for manufacturing industry share multiple equilibria associated with the case set 1 MDPs. Column 1 is the IV estimation of equation (12), which tests the null of a unique equilibrium by testing with the coefficient on the MDP shock is minus unity. Columns 2-5 present the results of the one, two, three, and four equilibria specifications of equation (17) with the threshold values that maximized the Schwarz criterion. The intercept ordering criterion requires: $\delta_1 + \delta_2 < b_1 < \delta_2 < b_2 < \delta_2 + \delta_3 < b_3 < \delta_2 + \delta_4$, where the constant is δ_2 above. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table D6: Case set 2 manufacturing industry share multiple equilibria test

	$s_{c(j),20} - s_{c(j),5}$	$s_{c(j),20} - s_{c(j),-1}$			
	IV Estimate (1)	1 Equilibrium (2)	2 Equilibria (3)	3 Equilibria (4)	4 Equilibria (5)
PreMDP Growth Rate	-0.002111				
	(0.008214)				
MDP Shock	8.694e-05***	-1.553	-4.2830	-5.503	-8.199
	(1.468e-05)	(11.50)	(89.78)	(103.5)	(129.2)
δ1			-20,510***	215.6	-631.7
			(7,345)	(8,566)	(16,390)
δ3				20,540**	-634.5
				(7,576)	(14,520)
δ4					20,550**
					(7,757)
Constant	0.1832	324.5	20,540***	1.140	636.1
	(0.2371)	(326.3)	(6,839)	(2,969)	(14,190)
Thresholds					
b_1			23.34	-0.5184	-8.618
b_2				60.57	0.2002
b_3					60.57
Intercept ordering criterion	N/A	N/A	Fail	Fail	Fail
Schwarz Criterion	N/A	-270.7	-335.2	-338.6	-342
Counties	30	30	30	30	30
R-squared		0.001	0.225	0.225	0.225

NOTES: The table presents the results of our tests for manufacturing industry share multiple equilibria for case set 2 MDPs. Column 1 is the IV estimation of equation (12), which tests the null of a unique equilibrium by testing with the coefficient on the MDP shock is minus unity. Columns 2-5 present the results of the one, two, three, and four equilibria specifications of equation (17) with the threshold values that maximized the Schwarz criterion. The intercept ordering criterion requires: $\delta_1 + \delta_2 < b_1 < \delta_2 < b_2 < \delta_2 + \delta_3 < b_3 < \delta_2 + \delta_4$, where the constant is δ_2 above. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table D7: Case set 3 manufacturing industry share multiple equilibria test

	$s_{c(j),20} - s_{c(j),5}$	$s_{c(j),20} - s_{c(j),-1}$			
	IV Estimate (1)	1 Equilibrium (2)	2 Equilibria (3)	3 Equilibria (4)	4 Equilibria (5)
PreMDP Growth Rate	7.444e-04	-0.02986	0.4695	1.076	1.118
	(0.009371)	(2.405)	(20.67)	(20.60)	(21.05)
MDP Shock	8.804e-05***				
	(2.746e-05)				
δ_1			-2,655***	-3.838	16.42
			(1,009)	(717.2)	(1,613)
δ_3				3,915***	18.83
				(1,263)	(1,639)
δ_4					3,915***
					(1,265)
Constant		41.55	2,654***	0.8327	-18.03
		(39.67)	(941.8)	(549.4)	(1,547)
Threshholds					
b_1			59.52	5.329	4.033
b_2				86.36	5.307
b_3					86.36
Intercept ordering criterion	N/A	N/A	Pass	Fail	Fail
Schwarz Criterion	N/A	-1887	-2408	-2412	-2417
Counties		250	250	250	250
R-squared		0.000	0.028	0.044	0.044

NOTES: The table presents the results of our tests for manufacturing industry share multiple equilibria for case set 3 MDPs. Column 1 is the IV estimation of equation (12), which tests the null of a unique equilibrium by testing with the coefficient on the MDP shock is minus unity. Columns 2-5 present the results of the one, two, three, and four equilibria specifications of equation (17) with the threshold values that maximized the Schwarz criterion. The intercept ordering criterion requires: $\delta_1 + \delta_2 < b_1 < \delta_2 < b_2 < \delta_2 + \delta_3 < b_3 < \delta_2 + \delta_4$, where the constant is δ_2 above. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table D8: Case set 4 manufacturing industry share multiple equilibria test

	$s_{c(j),20} - s_{c(j),5}$	$s_{c(j),20} - s_{c(j),-1}$			
	IV Estimate (1)	1 Equilibrium (2)	2 Equilibria (3)	3 Equilibria (4)	4 Equilibria (5)
PreMDP Growth Rate	-0.002376	-0.001606	-0.002849	-0.003307	-0.003346
	(0.009062)	(0.009231)	(0.003995)	(0.004011)	(0.004013)
MDP Shock	-0.03428				
	(0.1258)				
δ_1			-2.368***	-1.003*	0.4592
			(0.4885)	(0.6017)	(0.8238)
δ_3				1.910**	1.451
				(0.7861)	(0.9758)
δ_4					1.900**
					(0.7784)
Constant	0.5476	0.8182**	2.538***	1.137**	-0.3042
	(0.4252)	(0.4056)	(0.4513)	(0.5717)	(0.7990)
Thresholds					
b_1			2.051	1.512	1.153
b_2				3.017	1.512
b_3					3.017
Intercept ordering criterion	N/A	N/A	Pass	Fail	Fail
Schwarz Criterion	N/A	-839.9	-632.2	-636.1	-641.4
Counties	250	250	250	250	250
R-squared		0.000	0.087	0.098	0.100

NOTES: The table presents the results of our tests for manufacturing industry share multiple equilibria for case set 4 MDPs. Column 1 is the IV estimation of equation (12), which tests the null of a unique equilibrium by testing with the coefficient on the MDP shock is minus unity. Columns 2-5 present the results of the one, two, three, and four equilibria specifications of equation (17) with the threshold values that maximized the Schwarz criterion. The intercept ordering criterion requires: $\delta_1 + \delta_2 < b_1 < \delta_2 < b_2 < \delta_2 + \delta_3 < b_3 < \delta_2 + \delta_4$, where the constant is δ_2 above. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.